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A STUDY OF THE BEHAVIOR OF THE CROW *CORVUS AMERICANUS* AUD. BY THE MULTIPLE CHOICE METHOD

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We have previously reported in this Journal[2] observations on the behavior of crows in certain forms of visual discrimination. The subjects of that investigation were transferred from the Franklin Field-Station in September, 1913, to the Laboratory of Animal Psychology in Cambridge, and were there kept until June, 1914, in a cage approximately six feet in its several dimensions. Despite their close confinement and the lack of an out-of-door fly, the birds continued in excellent health and proved themselves able to withstand wholly satisfactorily the conditions of laboratory life. When returned to the Field-Station, they were considerably less tame than during the previous summer. For this reason they were not used further for experimental purposes, but were kept for general observations. Young crows were captured for the experiments which are reported in this paper.

Instead of following up the study of visual discrimination, we devoted our attention, during the summer of 1914, to an attempt to analzye ideational and allied forms of behavior in the crow by means of the Yerkes multiple choice method, and to the accumulation of additional facts concerning the natural history, instincts, and general habits of the birds.

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[1] The observations reported were made chiefly by Mr. Coburn and the paper was written by Mr. Yerkes.

[2] Coburn, C. A. The behavior of the crow *Corvus Americanus*, Aud. *Journal of Animal Behavior*, 1914, **4**, 185-201.

On June 7th, 1914, three young crows were captured near the Station. These birds were about ready to leave the nest. One, indeed, was taken from a limb beside the nest. This individual from the first exhibited fear and was so troublesome that after two days it was discarded and the remaining two birds were kept for observation. They were placed in a box which was frequently passed by human beings, and were several times a day fed by hand, being allowed to come out of the box at will and become thoroughly accustomed to the experimenters. From the time of capture they were perfectly tame, ate readily, and the characteristic fear reactions never appeared. When taken from the nest, they were probably at least six weeks old.

Throughout this report, these birds will be referred to as number 3 and number 4. Number 3 was from the first the larger of the two and the less timid. It, during the several months of observation, always came to us, perching on arms, shoulder, or head, as it had opportunity, and showing a friendly interest which was apparently somewhat independent of its desire for food. It evidently liked to be petted. Our assumption is that this bird is a male.[3] Number 4, by contrast, was smaller, shyer, more wary, and after a few weeks ceased to come to either of us, except as drawn by hunger, and even then it often hesitated to perch upon the hand or arm. In all probability, it is a female. It has eaten less than number 3, and has been considerably more difficult to experiment with. Usually, in the course of an experiment, if the birds were in competition, number 4 would stand aside for number 3.

Our additional experience with crows during the present season but emphasizes our conviction that they are among the most interesting of birds, and that their behavior is in every respect worthy of careful analytic study. With respect to what we shall term "ideational behavior," they have fallen short of our expectations, for in the light of their varied interests, ingenuity, curiosity, ceaseless activity, and apparent insight into simple situations, we had assumed that they possess an intelligence equal to that of many of the more intelligent mammals. The experiments now to be reported were conducted for the special purpose of obtaining definite and reliable information concerning the nature and limitations of their ability to adjust themselves to certain fairly simple, although novel, situations.

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[3] Since this was written, dissection has definitely established our surmise in the case of both birds.

We sought to make our measurements of intelligence by a method recently devised at the Psychopathic Hospital, Boston, by R. M. Yerkes, for the comparative study of ideational and allied forms of behavior in man and other animals. This method has been named the soluble-problem multiple-choice method. It was devised primarily for the purpose of enabling the comparative psychologist to present to any human or infra-human subject, no matter what the age, degree of intelligence, or condition of normality or abnormality, a series of situations increasing in complexity from an extremely simple one to one so intricate that even the most intelligent human subject might spend hours or days in adjusting himself to it. By means of this multiple choice method, it is hoped and confidently expected that the materials of comparative psychology may be rapidly increased and the analyses of animal behavior be made invaluable to the psychopathologist.

A general description of the method should preface this account of the special form in which it was applied to the crow, inasmuch as only a very brief account of it has been published.[4]

In brief, the essentials of the method are these. A series of reaction mechanisms, appropriate to the subject, are presented. From this series one mechanism must be selected which, when properly approached, will yield the subject the satisfaction of success and, possibly, the reward of food. With each presentation of the reaction mechanisms, they are varied in number and in position. The subject is therefore forced to select the proper mechanism on the basis of some particular relationship of that mechanism to its fellows, this relationship having been determined upon in advance by the experimenter. It may be, for example, such a simple relation as first at the left of the series as the subject approaches, or first at the right of the series, or second at the left, or alternately the first at the left and the first at the right, or the middle of the series. Imagine, then a series of piano keys which may be presented to a human subject. They may vary in number from two to twelve (this was the original form of apparatus). Some one key, in any group of keys presented, when pressed will cause a bell to ring, thus indicating success. Without other aid than his own observation, the subject is expected, from repeated presentations of the keys, to discover the essential relation and to acquire the ability to select the right key with certainty.

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[4] Yerkes, Robert M. The study of human behavior. *Science*, 1914, **39**, 625-633. In this paper the writer describes his method in contrast with the Hamilton quadruple choice method.

This method has the advantage of enabling the experimenter to present increasingly difficult problems to his subjects. It has further the advantage of enabling him conveniently to record the essential features of reaction, and later to analyze the reactions at his leisure. But most important of all, it yields strictly comparable results when applied to widely differing organisms. Naturally, although the same problems may be presented to diverse types of organism, the reaction mechanisms must be suited to the subject in question.

Without further general comment or discussion of the multiple choice method, we shall describe the form of apparatus and procedure employed with the crow.

APPARATUS AND METHOD

In the accompanying plate, designated as figure 1, and in the ground plan of the observation-room and apparatus, shown in figure 2, the general experimental situation is represented. Figure 1 shows in the background the building which was used both as a shelter for the crows and as an observation place for the experimenter. To this building is attached a fly which appears in C, D, and E of Figure 1. In figure 2, the ground plan of the building, are seen the experimenter's room, A, and the crow room, B, the latter containing a perch, P. All coarsely dotted lines in this figure indicate walls or partitions made of poultry wire. The large fly was, for the purposes of our experiment, divided into two parts by a wire partition. In the smaller of these portions, shown at the right of figure 2, the multiple choice apparatus was located. The crows could enter this portion of the fly only at the will of the experimenter, whereas they were allowed the freedom of the larger portion, which we have labelled C. As figure 2 is drawn to scale (one inch to forty-eight) it is unnecessary to give the measurements of the

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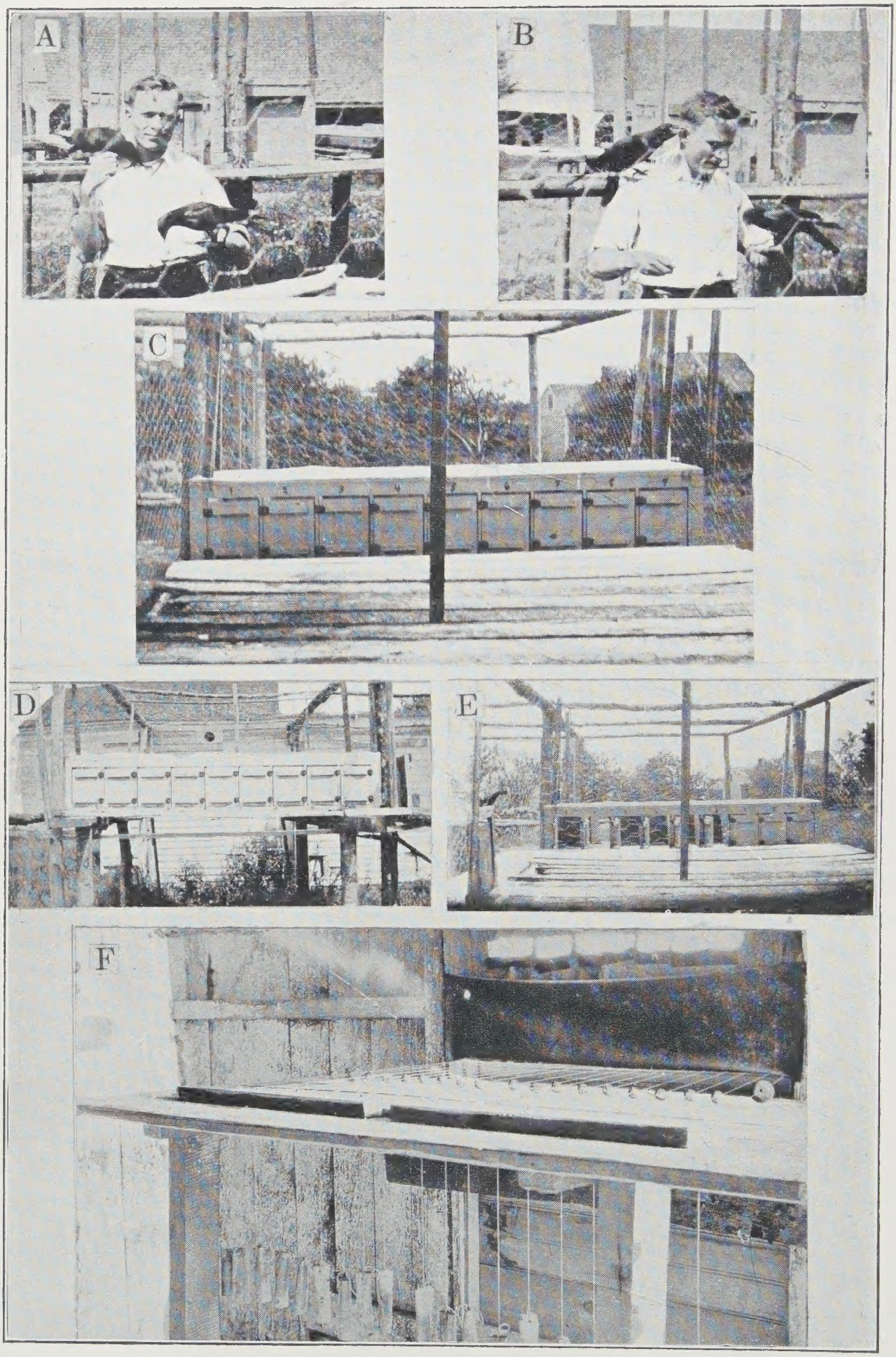

FIGURE 1. Views of crows and apparatus for multiple choice experiments. A and B, crows, number 3, ♂, on shoulder of experimenter, and number 4, ♀, on arm; C, the multiple choice box seen from the observer's room and from the direction of approach by the crow, the compartments are numbered 1 to 9 and below each number is an entrance door. D, the same seen from the opposite side or rear, with the nine exit doors closed. E, the box seen from the observer's room, with entrance doors 1 to 6 and exit door 2 open. At the extreme left, above the entrance door to the experiment compartment of the fly, one of the crows is visible. F, the observer's table, showing curtain before window (partially drawn aside to admit light for the camera) and the weighted cords with pull buttons for opening and closing doors.

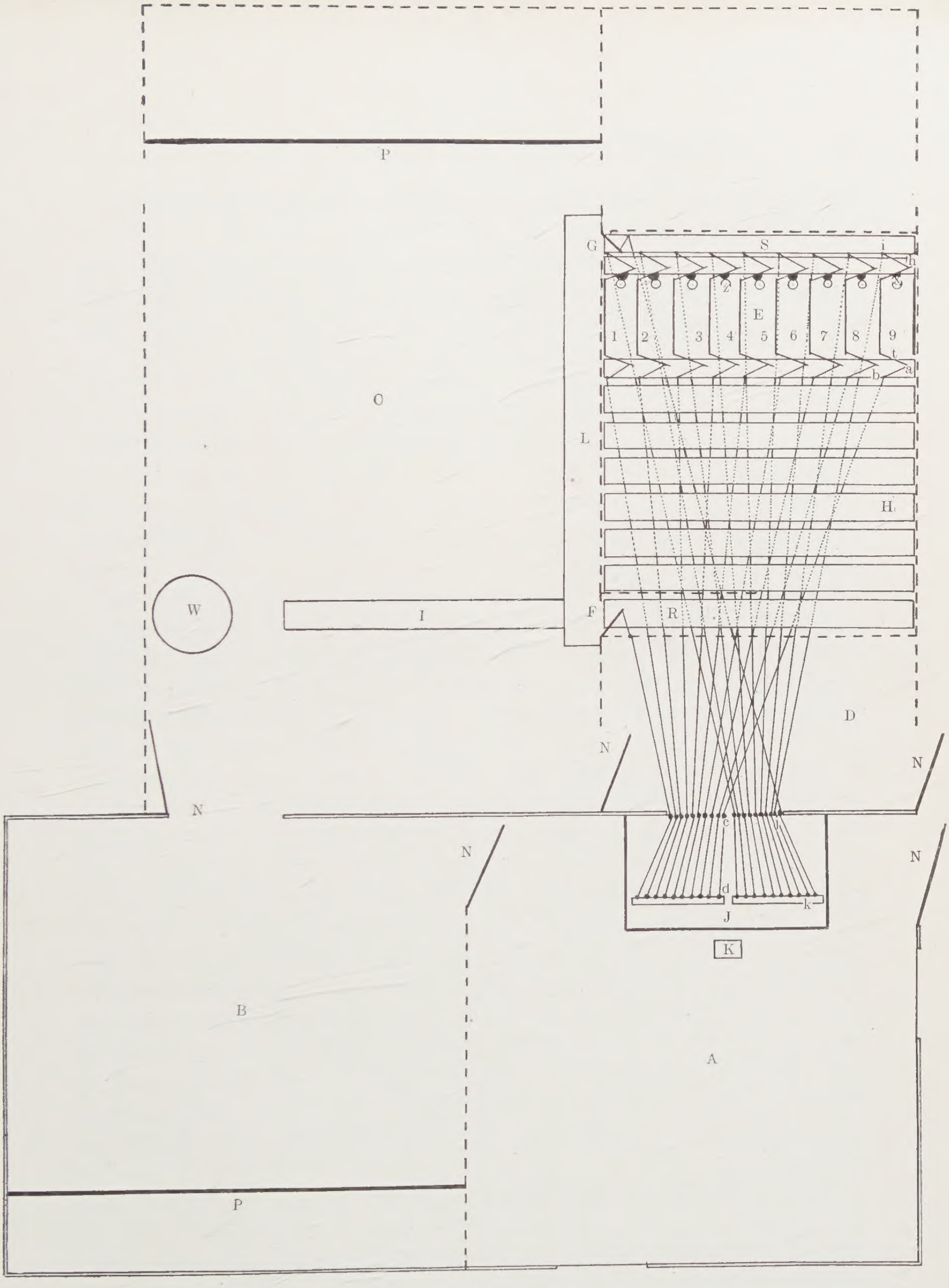

FIGURE 2. Ground plan of crow house, fly, and apparatus. Scale, $\frac{1}{48}$. A, observation room; B, bird room; C, main portion of fly; D, passageway for experimenter; E, multiple choice box; F, entrance door between main fly, C, and alley to reaction chamber, H; G, exit door between alley S and main fly; H, reaction chamber, the floor boards of which are separated somewhat; I, L, approaches to the doors F and G; J, observer's table and key-board; K, observer's stool; N, doors for experimenter's use; P, perches; R, alley leading to middle of reaction chamber H; S, alley leading from exits to main fly; W, water tub for crows.

Numerals 1 to 9, compartments of multiple choice box; a, attachment of cord to entrance door, t, of compartment 9; b, screw eye for cord; c, screw eye at entrance to observer's room; d, wooden button on cord; under d is a small brass pulley for cord; h, i, j, k, indicate course of cord from exit door of compartment 9 to key-board; x, metal cover for food receptacle of compartment 9; z, food receptacle of compartment 4.

building and fly. We shall give a more detailed description of the experimental device.

The latter is shown fairly well from different points of view in the parts of figure 1. Figure 1 C is a view of the multiple choice box from the front, that is, the side of approach by the subject. All the entrance doors are closed. Figure 1 E shows the apparatus from the same point of view, with the entrance doors 1 to 6 and the exit door 2 open. Figure 1 D, instead, shows the apparatus from the opposite side, with the several exit doors closed.

By referring now to both figures 1 and 2, we should be able to obtain a clear idea of the construction of the experimental mechanism and its use.

The multiple choice box, as we shall call it, appears in ground plan as E of figure 2. It is divided into nine like compartments, each with a door at both ends, opening outward. The outside measurements of the multiple choice box are 81 inches long by 20 inches wide by 15 inches high. The frame of the box is made of 2 by 2 inch stock, and the floor, ends, partitions, and doors, of half-inch stock. The top, which is hinged for convenience of access, consists of wire netting, 1¼ inch mesh, on a wooden frame. On the inside, each of the nine compartments is 19 inches long by 8 inches wide by 13 inches high. The entrance and exit doors are 9½ inches high by 7⅝ inches wide. All of the doors are mounted with spring hinges which hold them shut. On the lower inner edge of each exit door is a piece of tin (x) which, when the door is closed, projects 2 inches into the compartment and covers a hole (z) in the floor of the compartment 1½ inches in diameter by ⅞ inches deep. These metal covers, as well as the holes, are represented in the ground plan of the apparatus, figure 1, x and z. The use of these holes is to contain food which serves as a reward for the bird when the exit doors are opened.

The system of entrance and exit doors, nine of each, and also the main entrance door, labelled F in figure 2, and shown in the extreme lower left corner of figure 1 E, and the main exit door, labelled G in figure 2, and shown at the right end of the multiple choice box in figure 1 D, are controlled from the experiment room A by a system of cords passing through screw eyes and pulleys.

These cords are indicated by dotted lines where they pass under the floor of the multiple choice box or under the boards which serve as an approach to the box: Elsewhere they appear as solid lines. The arrangement of the cord-system within the experiment room is rather unsatisfactorily shown in figure 1 F.

On a table, J, before which the observer sits on the stool, K, are two groups of cords, each with a wooden button attached in a convenient position. The group at the experimenter's left consists of the cords connected with the ten entrance doors, and the group at the right, similarly, of those connected with the ten exit doors.

We may now trace the course of the cords from the doors of compartment 9. A cord is fastened at a to the lower outer corner of the entrance door t. It thence passes through the screw eye b in the edge of the approach board. From this point it extends, under the interrupted floor of the reaction chamber H, to a screw eye, c, in a block across the aperture leading to the experiment room. Thence the cord passes over a small brass pulley at d and through a hole in the table J. (In figure 2 the pulley is hidden by the wooden button on cord.) It is kept taut by a lead weight under J. Similarly, the cord for the exit door of compartment 9 is attached to the lower outer corner of the door at h, passes through the screw eyes, i and j, to the pulley k, and is kept taut by a leaden weight. The cords for the main entrance and exit doors, F and G, run to the extreme left and right respectively of the experimenter's table.

The experimenter operates a door by grasping the wooden button shown on each cord in figure 1 F and pulling it toward him. When he has pulled as far as the button will come, the door to which the cord is attached stands wide open, and the leaden weight under the table serves to hold it in this position as long as the experimenter desires. When he wishes it closed, he simply pushes the button back to its former position, and the strength of the spring hinges suffices to overcome the pull of the weight.

In order that the bird should not see and be influenced by the movements of the experimenter, a black curtain was hung before the opening into the experiment room, and through small holes cut in it, the experimenter was able to observe the movements of his subject. At no time during the investigation did the crows give evidence of noticing the experimenter when they were reacting.

The remaining features of the apparatus will be mentioned in connection with the following brief description of the experimental procedure. In preparation for a series of trials, the experimenter opens each of the exit doors and places in each food container a small bit of milk-soaked bread. He then closes the exit doors, thus covering the food, and takes his place at K. He next opens a group of entrance doors. Let us suppose, as is shown in figure 1 E, that the doors numbered 1 to 6 are opened, and, further, that the compartment which may be designated as the correct one is the first at the subject's left, that is number 1. Having made these preparations, the experimenter, by means of the proper cord, opens the main entrance door F, and the bird, either by walking up the approach board I or by alighting on the approach board L, on a level with the entrance door, is immediately able to enter the reaction chamber H, by way of the alley R. By two wire partitions which appear as dotted lines in figure 2, it is forced to walk straight ahead until it reaches the center of compartment H. It may then face and, if it so chooses, directly approach the central compartment of the multiple choice box. But under the circumstances, with entrance doors 1 to 6 open, it would naturally swerve toward the left.

In case it enters compartment 1, the experimenter quickly and noiselessly closes the entrance door after it, by releasing the appropriate cord, and immediately thereafter, opens the exit door of the compartment by pulling on the appropriate cord. He, thus, with one hand prevents the retreat of the bird from the compartment and with the other uncovers the food, so that the bird may obtain the reward for a correct reaction. As soon as the food has been swallowed, the crow steps out of the compartment, the exit door is closed by the experimenter, and the bird either immediately, or at the experimenter's pleasure, is allowed to return to the fly C by way of the main exit door G.

If, instead of choosing the right compartment, the crow enters some other one, the procedure is different. Immediately upon its entrance, the experimenter closes the entrance door. He then, with a stop-watch, measures a definite period during which the bird is confined in the compartment. This period was varied during our experiments from 15 to 60 seconds, in an attempt to discover the most satisfactory length of confinement. At the proper moment, the experimenter opens the entrance door and the crow is allowed to retrace its steps. It may then immediately make another choice. But not until it enters the right compartment, is it awarded with food and allowed to return to the fly. Thus punishment for incorrect choices is combined with reward for correct choices.

In further description of the apparatus, it should be said that wire partitions at each side of the multiple choice box, and extending from the lid of the same to the roof of the fly, prevented the crow from walking or flying over the box, while boards both in front of the box and behind it and on a level with its floor form a floor which prevented the bird from getting under the box. The only possible course for the subject from main entrance to main exit door is by way of one of the compartments.

Experience shortly indicated that the crows could be used most satisfactorily if given their trials alternately, and the method finally settled upon was that of admitting one crow to the apparatus, allowing it to make its choice, and then holding it in the passageway beyond the exit doors until the other crow had passed through the main entrance door into the reaction chamber. Thus, as one subject emerged from a compartment of the box E, the other bird entered the reaction chamber. When, as sometimes happened, the one or the other bird failed to respond immediately and appropriately and both were in the fly, it was fairly easy for the experimenter to admit the proper bird by carefully manipulating the entrance door.

PRELIMINARY TRAINING

The crows obtained almost all of their food in the multiple choice box. In order that they should work steadily and industriously, it was necessary to have the pieces of bread or mouse meat, which was sometimes used instead of bread, very small. It proved possible to obtain as many as twenty reactions per day from each bird, in series usually of five each.

We shall now consider the course of experimentation and its results. One June 21st, the crows having attained ability to feed themselves, preliminary training was undertaken, and from that time they were fed in the multiple choice box. They exhibited no fear, rapidly became familiar with the apparatus, and acquired skill in making the trip from the main fly, through the experiment compartment, back to the fly. For several days, both the entrance and the exit doors of the compartments were kept open. Then the situation was changed by the closing of the exit doors, and the crows were trained to enter a compartment and wait for their food.

On June 27th, the first series of trials worthy of special mention was given. The apparatus was in perfect working condition. Food was placed *on the floors* of the several compartments; the exit doors were closed and the entrance doors were open. The main entrance door was opened, and both birds were allowed to enter the reaction chamber and go to the compartments for food. As they entered the compartments, the exit doors were opened and the entrance doors closed. Thus, by a series of trials they were habituated to the opening and closing of the doors and were taught to make the circuit promptly from the main fly back to the same by way of the multiple choice box.

On the following day, June 28th, the food was placed in the food containers and the exit doors were closed. Number 3 entered the compartments rapidly and made the circuit usually without delay, but number 4 at first refused to enter the compartments. Within two days, it, however, was readily entering, in its search for food.

On June 28th, only three or four of the entrance doors to the compartments were opened at any one time. In the previous preliminary training all of the doors had been opened. Neither bird showed any marked preference for a particular compartment in the multiple choice box.

On June 30th, the method was tried of confining one of the crows in the crow room B, of figure 2, while the other was given its trials. Later in the day, the birds were given another series of trials alternately, the one being kept in the exit alley as described on page 83, until the other had entered the reaction chamber. This method proved satisfactory and was later employed to the exclusion of the former.

Up to this point, the two subjects adapted themselves to the different situations with almost equal rapidity. Number 4 was somewhat less willing to try new things than number 3, and seemed to be hampered by its shyness.

A significant incident is the following. In one of the trials, number 4 accidentally fell through a five inch space which had been left before the entrance doors in order that the crow should not too closely approach a compartment unless it intended to enter it. The bird fell to the ground beneath the apparatus, finding there some pieces of bread which had been dropped earlier in the day. Naturally enough, it ate them before it could be induced to return to the fly. Ever thereafter, until this crack had been closed, this bird, as it approached the compartments, would look through the crack to the ground. Several times it flew down in search of food.

RESULTS, PROBLEM 1

With the final series of trials given on June 30th, regular experiments were initiated. *The problem which the birds were required to solve was that of learning to select the first open door at the right.*

Ten settings as we shall call them, were chosen by the experimenters. These are given below, numbered 1 to 10. After each number appears the series of open doors; in the next column, the total number of doors open; and finally in the last column, the number of the right compartment in which the reward of food might be obtained.

PROBLEM 1. First door at the subject's right to be chosen

Settings	Doors open	No. of doors open	No. of right door
1	7.8.9	3	9
2	2.3.4	3	4
3	3.4.5.6.7	5	7
4	1.2	2	2
5	2.3.4.5.6	5	6
6	6.7.8	3	8
7	3.4.5	3	5
8	2.3.4.5.6	5	6
9	1.2.3	3	3
10	7.8.9	3	9

In this series of ten settings, a total of thirty-five doors were open, of which number, ten were of course "right doors." Consequently, the chance of a selection of the right door, without previous experience or trial, is one to two and one-half.

In general, it was the purpose of the experimenter, as far as possible, to follow through this series of settings from 1 to 10, and then to return to the beginning and repeat the series. No matter how many trials in succession could be given, the experiments were resumed at the point of interruption of the regular series of settings. Thus, if five trials were given, beginning with setting 1 and extending through setting 5, the next series would begin with setting 6 and continue through setting 10.

As a matter of convenience, it was also decided to have the two crows work on different settings. For example, while crow 3 was presented with the settings 1 to 5, crow 4 would be presented with settings 6 to 10. This enabled the experimenter to avoid the necessity of refilling the food containers after each trial, and it also prevented the crows from developing the tendency to follow one another by sensory cues.

After a very few days of experimentation, both birds reacted with remarkable alacrity and facility. They were, as a rule, prompt to enter the reaction area and almost as prompt to leave the exit area.

In the initial regular experiments, thirty seconds confinement in the wrong compartment was used as punishment for mistakes. But it shortly appeared that this was too long an interval, for the birds hesitated to enter any of the compartments after a half minute confinement in one of them. It was therefore decided to use the period of fifteen seconds as punishment for incorrect choices. Especially during the early experiments, the crows often exhibited considerable fear and excitement when shut in the small compartments. This diminished toward the end of our work, and it was then possible to confine them for a half minute or even a minute without causing disturbing excitement.

The experimenter kept, as a matter of routine, a record of the time from admission to the reaction chamber to entrance into the right compartment. There is no special reason to consider these records significant, and we shall omit them from this report.

Careful record was also kept of the chief features of the behavior of the bird during this interval. The simple system of symbols, which appears below, was adopted for this purpose.

○, to center of the reaction area
┌, to left hand far corner of the area
┐, to right hand far corner of the area
└, to left hand near corner of the area
┘, to right hand near corner of the area
⊔, to center of the near side of the area
⊏, to center of the left side of the area
⊐, to center of the right side of the area

If the crow merely looked into one of the compartments without entering, the number of the compartment was recorded.

If it, instead, entered a compartment, the number was underscored. In case the compartment entered happened to be a "wrong one", the time of entrance was placed in parenthesis immediately after the number of the compartment. When the time exceeded a minute, the number indicating the minutes was placed in a circle. For less frequent forms of behavior, other provisions were found convenient, and by the use of symbols and other abbreviations it was found easy to obtain a fairly complete description of the subject's behavior.

To illustrate the use of the above symbols, the following record of a trial (trial 32 of number 3 on July 23), setting 1.**2**, is presented.

> 4, 3, └, ┘, └, ①, └, O, └, 1, └, ②, (trying to get out of area),
> └, ┘, ③, ┘, └, ┘, pkd. at hole in floor), ④, 1, O, └, 9, ⑤, ⑥,
> ⑦, O, ┌, └, 2, ⑧, 1, **2**, 8′ 13″.

In this trial, crow number 3 did not enter the wrong compartment at all. The time between the fifth and the seventh minutes was spent before compartment 9.

It was decided by the experimenters that when a crow had made ten correct choices in succession, its training should be considered complete, or in other words, it should be said to have solved the problem.

In the case of the problem in question, crow number 3 at the end of thirty-two trials had entered the right compartment twelve times in succession, but in several of these trials it had been aided by the experimenter, who moved the exit door slightly in order to attract the attention of the bird after it had for several minutes refused to enter any compartment.

In the accompanying table 1, a summary of the trials for each of the birds in problem 1 appears. At the head of the several columns are the settings numbered 1 to 10, with the right number in bold faced type. In the first column at the left, under each of the several settings, appears the number of the trial and the series of compartments entered. Thus, for example, referring to the results for crow number 3, in trial number 5, which was the first trial in the regular series, the bird entered compartment 8 and then compartment 9. In trial 6, it immediately entered compartment 4, the right one. The letter a, following a number, indicates that the bird was aided in its choice by the experimenter.

It is possible by careful study of this and succeeding tables to discover the reactive tendencies of the organism, and to note both the appearance and the disappearance of the same.

Problem 1, the first door at the right, proved a very easy one for both crows. It was mastered by number 3 after fifty-five trials, and by number 4 after fifty-one trials.

Table 2 presents the results of the various series of trials, ranging in number from three to five for each subject. The number of successes and failures in each series and the ratio of successes to failures for each day appear. The letter R in this table indicates correct first choices, the letter W, incorrect first choices. The table has to do only with first choices.

In contrast with the above results in problem 1, the first door at the right, we present in table 3 a summary of the results for problem 1a, the first door at the left, the trials for which were given not immediately after those just described but at the end of the season, and after the crows had for several weeks worked on problem 2, the second door at the left. Naturally the influence of their training to go to the second door retarded the formation of the habit of choosing the first door at the left. For the satisfactory solution of the problem, one hundred trials were required by each bird. Doubtless a change of experimenters after trial 75 somewhat delayed progress. The results which appear in tables 3 and 4 demand no further comment.

Recurring now to problem 1, it is obvious that from the human point of view this is a very simple problem. The crows solved it readily, but in the course of their work they frequently experienced discouragement and were aided in a considerable number of their early trials by the experimenter. Doubtless our results would be more significant had this aid been withheld, but at the outset of our work we hesitated to run the risk of spoiling our subjects by over-discouraging them. In problem 1a, no aid was needed.

Varied reactive tendencies do not appear in connection with this problem. Very few wrong choices were made. Consequently, all that can be gleaned from the results is a general knowledge of the behavior of the crow in the face of a certain fairly simple experimental situation.

TABLE 1

RESULTS FOR CROW NUMBER 3 IN PROBLEM 1

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	7.8.9	T.	2.3.4	T.	3.4.5.6.7	T.	1.2	T.	2.3.4.5.6	T.	6.7.8	T.	3.4.5	T.	2.3.4.5.6	T.	1.2.3	T.	7.8.9
5*	8.9	6	4	7	7a	8	2												
						9	2	10	4.6	11	8	12	5	13	6	14	3	15	8.9a
16	9a	17	4a	18	7a	19	2a	20	**	21	8a	22	5a	23	6				
														24	6	25	3	26	9
27	9	28	4																
29	9	30	4a	31	7	32	2	33	5.6	34	8	35	4.5	36	6	37	3	38	9
39	8.9	40	4	41	7														
42	8.9	43	4	44	6.7	45	1.2	46	6	47	8	48	5	49	6	50	3	51	9
52	9	53	4	54	7	55	2	56	6										

* Records for first four trials:
(1) 1.2.**3** (2) 8.**9** (3) 6.7.**8** (4) 3.4.5.6.**7**
3 9 6.8a 3.7a

** Blanks indicate failures to choose.

RESULTS FOR CROW NUMBER 4 IN PROBLEM 1

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	7.8.**9**	T.	2.3.**4**	T.	3.4.5.6.**7**	T.	1.**2**	T.	2.3.4.5.**6**	T.	6.7.**8**	T.	3.4.**5**	T.	2.3.4.5.**6**	T.	1.2.**3**	T.	7.8.**9**
*																			
										5	6.7.8a	6	5	7 2	.6a	8	—		
																9	3	10	9
11	8.9a	12	4a	13	7	14	2												
						15	2	16	6	17	8	18	5	19	4.2.6	20	3	21	9
22	8.9	23	4	24	4.7	25	2	26	6	27	7.8	28	5						
										29	6.8	30	5	31	6	32	3	33	9
34	9	35	3.4	36	7			37	6										
38	8.8.7.9																		
												39	3.5	40	6	41	3		
										42	8	43	5	44	6	45	3	46	9
47	9	48	4	49	7	50	2	51	6										

* Records for first four trials:
(1) 1.2.**3** (2) 8.**9** (3) 3.4.5.6.**7** (4) 2.3.4.5.**6**
3 9 5.5.7a 6

TABLE 2

DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT FIRST CHOICES

PROBLEM 1

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 30	4	2	2	2	2	1: 1	June 30	4	3	1	3	1	1:.33
July 1	3	2	1				July 1	3	1	2			
“ “	1	1	0				“ “	1	0	1			
“ “	3	2	1				“ “	3	2	1			
“ “	3	3	0	8	2	1:.25	“ “	3	2	1	5	5	1: 1
“ 2	2	0	2				“ 2	2	2	0			
“ “	4	0	4	0	6	0: 6	“ “	4	3	1	5	1	1:.20
“ 3	3	1	2				“ 3	3	2	1			
“ “	5	5	0				“ “	5	3	2			
“ “	4	3	1				“ “	4	3	1			
“ “	4	2	2	11	5	1:.45	“ “	4	2	2	10	6	1:.60
“ 4	5	4	1				“ 4	5	4	1			
“ “	5	3	2				“ “	5	5	0			
“ “	5	5	0	12	3	1:.25	“ “	5	5	0	14	1	1:.07
“ 5	5	5	0	5	0	1: 0							

TABLE 3

Results for Crow Number 3 in Problem 1a

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	**1**.2.3	T.	**7**.8.9	T.	**3**.4.5.6.7	T.	**8**.9	T.	**2**.3.4.5.6	T.	**6**.7.8	T.	**5**.6.7	T.	**4**.5.6.7.8	T.	**7**.8.9	T.	**1**.2.3
1	3.2.3.1	2	8.9.8.7	3	3	4	9.8	5	3.2	6	6	7	5	8	5.4	9	8.8.8.8.7	10	3.2.3.2 2.2.1
11	3.2.1	12	8.7	13	3	14	9.8	15	2	16	6	17	5	18	4	19	8.8.7	20	3.2.2.2.1
21	3.2.2.1	22	8.7	23	3	24	9.9.8	25	2	26	6	27	5	28	4	29	8.8.8.7	30	2.1
31	3.2.3.1	32	8.7	33	3	34	8	35	2	36	6	37	5	38	4	39	8.8.8.8 8.8.7	40	2.3.3.1
41	2.2.3.1	42	7	43	3	44	8	45	2	46	6	47	5	48	4	49	8.7	50	2.3.1
51	2.1	52	8.8.7	53	3	54	8	55	2	56	6	57	5	58	4	59	7	60	2.1
61	2.3.2.2 2.2.1	62	7	63	3	64	8	65	2	66	6	67	5	68	4	69	7	70	2.1
71	2.1	72	7.	73	3	74	8	75	2*										
	7.8.9		**6**.7.8.9		**2**.3.4.5 6.7		**4**.5.6.7		**1**.2.3.4 5.6		**5**.6.7.8 9		**1**.2.3.4 5.6.7.8.9		**3**.4.5.6 7.8		**7**.8.9		**2**.3.4.5
76	8.7	77	8.6	78	2	79	7.4	80	1	81	5	82	2.1	83	3	84	8.7	85	3.2
86	7	89	6									90	1	87	3			88	2
91	1	92	7	93	3	94	8	95	2	96	6	97	5	98	4	99	7	100	1

RESULTS FOR CROW NUMBER 4 IN PROBLEM 1A

	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10
T.	**1**.2.3	T.	**7**.8.9	T.	**3**.4 5 6.7	T.	**8**.9	T.	**2**.3.4.5.6	T.	**6**.7.8	T.	**5**.6.7	T.	**4**.5.6.7.8	T.	**7**.8.9	T.	**1**.2.3
										1	7.8.6	2	5	3	8 7.6 6 7.5.6.7.5 7.6.8.4	4	7	5	3.2.3.1
6	3.2.1	7	7	8	3	9	8	10	2	11	6	12	5	13	4	14	7	15	3.2.1
16	2.3.1	17	8.8.7	18	5.3	19	8	20	4.2	21	7.8.6	22	5	23	4	24	7	25	3.2.3.1
26	3.2.3.1	27	7	28	3	29	8	30	4.5.6.3.2	31	6	32	5	33	4	34	7	35	3.2.2.1
36	3.1	37	7	38	4.5 3.	39	8	40	3.4.2	41	6	42	5	43	4	44	7	45	2.1
46	2.3.1	47	7	48	3	49	8	50	2	51	6	52	5	53	5.6.7.4	54	7	55	1
56	3.1	57	7	58	4.3	59	9.8	60	3.2	61	6	62	5	63	4	64	7	65	2.1
66	2.1	67	7	68	3	69	8	70	2	71	6	72	5	73	5.6.4	74	8.7	75	1.*
	7.8.9		**6**.7.8.9		**2**.3.4.5 6.7		**4**.5.6.7		**1**.2.3.4 5 6		**5**.6.7.8 9		**1**.2.3.4 5.6.7.8.9		**3**.4.5.6 7.8		**7**.8.9		**2**.3.4.5
76	8.7	77	6	78	2	79	4	80	1	81	7.5	82	1	83	6.3	84	7	85	2
		90	6			86	4	87	1	88	5							89	2
91	1	92	7	93	3	94	8	95	2	96	6	97	5	98	4	99	7	100	1

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* At this point a change in experimenters occurred. The settings were also changed as is indicated. Beginning with trial 91 the original series of settings was again used.

TABLE 4

Daily Series and Averages with Ratios of Correct to Incorrect First Choices

Problem 1a

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug. 8	5	1	4				Aug. 8	5	2	3			
" "	5	2	3				" "	5	4	1			
" "	5	2	3	5	10	1: 2	" "	5	4	1	10	5	1:.5
" 9	5	3	2				" 9	5	1	4			
" "	5	2	3				" "	5	3	2			
" "	5	3	2				" "	5	3	2			
" "	5	3	2	11	9	1:.81	" "	5	4	1	11	9	1:.82
" 10	5	3	2				" 10	5	2	3			
" "	5	4	1				" "	5	4	1			
" "	5	3	2				" "	5	4	1			
" "	5	3	2	13	7	1:.53	" "	5	4	1	14	6	1:.35
" 11	5	4	1				" 11	5	1	4			
" "	5	4	1				" "	5	4	1			
" "	5	4	1				" "	5	4	1			
" "	5	4	1	16	4	1:.25	" "	5	3	2	12	8	1:.66
" 13	5	2	3				" 13	5	4	1			
" "	5	2	3				" "	5	3	2			
" "	5	5	0	9	6	1:.67	" "	5	5	0	12	3	1:.25
" 14	5	5	0				" 14	5	5	0			
" "	5	5	0	10	0	1: 0	" "	5	5	0	10	0	1: 0

RESULTS, PROBLEM 2

Our second problem, which we arranged as a more difficult one than the first, proved for the crows much more difficult than we had expected. It may be described as *the problem of the second door at the left.* The series of ten settings for this problem is as follows:

Problem 2. Second door at the subject's left to be chosen

Settings	Doors open	No. of doors open	No. of right door
1	7.8.9	3	8
2	6.7.8.9	4	7
3	2.3.4.5.6.7	6	3
4	4.5.6.7	4	5
5	1.2.3.4.5.6	6	2
6	5.6.7.8.9	5	6
7	1.2.3.4.5.6.7.8.9	9	2
8	3.4.5.6.7.8	6	4
9	7.8.9	3	8
10	2.3.4.5	4	3

For these ten settings the total number of doors open is fifty, of which ten are "right doors." The chance of a correct first choice, without previous experience or the prejudical influence of training in problem 1, is one to four.

In this problem, the first fifty trials were given to the crows in groups of two and three each. The birds made so many mistakes at the beginning that they became discouraged, and after two or three trials, would refuse to work. Later, as they became accustomed to the situation and the experimenter increased the degree of hunger, they could be induced to react five times in succession. Consequently the number of trials per series was increased to five.

Now, as in the case of problem 1, the experimenter was forced, in order to avoid the possibility of utterly discouraging his subjects, to aid them after they had worked for several minutes without success in locating the right door. This was always done by slightly moving the exit door of the right compartment.

After sixty-one trials had been given, the period of punishment was increased from fifteen seconds to thirty seconds. The thirty-second interval was used up to the four hundred and sixtieth trial. It was then increased to sixty seconds, but as the crows refused to work, it was decreased after forty trials to fifteen seconds.

After the fiftieth trial, the series regularly consisted of five trials, and four series were, as a rule, given each day.

Table 5 presents for problem 2, as does table 1 for problem 1, a summary of the choices for each of five hundred trials given each crow. As in table 1, the settings are indicated at the top of the various columns, and under each setting appear the results of the various trials for that particular setting.

It appears from table 5 that number 3, in the case of setting No. 1, 7-9, failed with few exceptions in its first choices until the three hundred and forty-third trial, whereas thereafter it usually succeeded. On the contrary, in the case of setting No. 6, 5-9, we observe that the bird almost never succeeded in selecting the right compartment in the first trial. Moreover, there is absolutely no evidence of improvement.

TAB

RESULTS FOR CROW NU

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.**8**.9	T.	6.**7**.8.9	T.	2.**3**.4.5.6.7	T.	4.**5**.6.7	T.	1.**2**.3.4.5.6
1	9.8	2	9.7	3	7.6.7.7 7.7.3a	4	6.7.5a	5	6.2a
11	8a	12	7a	13	3	14	5a	15	2
21	9.9.9.7 9.8	22	9.7a	23	2.6.7.3a	24	4.7.4.4 4.5a	25	1.2a
31	7.9.8	32	6.9.7	33	4.3a	34	4.4.7.5	35	4.4.3.2
41	8	42	6.6.7	43	3	44	4.4.5	45	3.2
51	7.9.8	52	6.6.6.6 7	53	2.3	54	4.4.4.5	55	3.2
61	7.8	62	6.7	63	3	64	4.4.4.4 4.7.5	65	2
71	7.9.7.7 7.8	72	6.8.6.6 6.7	73	2.2.2.7 2.3	74	4.4.6.7 7.7.5	75	2
		67	6.7						
81	7.8	82	8.9.7	83	4.5.6.7 7.7.7.3	84	4.4.7.7 7.4.4.5	85	1.1.2
91	7.8	92	6.7	93	5.6.7.7.7.6 4.5.7.2.3	94	4.5	95	2
103	7.8	104	6.7	105	4.5.6.7.3	106	6.7.7.7.6 4.7.4.5	107	3.4.5.1.1.1 3.1.1.5.3.2
113	8	114	6.7	115	7.7.7.3	116	7.5	117	2
123	7.8	124	6.7	125	3	126	4.5	127	2
133	7.8	134	6.7	135	3	136	5	137	3.4.5.1.2
143	7.8	144	6.7	145	4.5.6.7.5 6.2.2.3	146	4.5	147	1.2
153	7.8	154	6.7	155	2.3	156	4.5	157	2
163	7.8	164	6.7	165	2.3	166	4.5	167	1.2
173	7.8	174	6.7	175	2.3	176	4.5	177	1.2
183	7.8	184	6.7	185	2.3	186	4.5	187	1.2
193	7.8	194	6.7	195	2.3	196	6.7.4.5	197	2
		204	6.7	205	3	206	4.5	207	3.4.5.6 1.2
213	7.8	214	6.6.7	215	2.3	216	4.5	217	2
223	8	224	6.7	225	4.5.2.3	226	4.5	227	3.4.5.1.2
233	7.8	234	6.7	235	3	236	4.5	237	2
243	8	244	7	245	2.3	246	7.5	247	1.2
253	7.7.7.7.7 7.7.7.7.7 7.7.7.8a	254	6.6.7	255	3	256	4.4.4.4 4.7.5	257	3.4.2
263	8	264	8.7	265	3	266	4.6.7.5	267	2
273	7.7.8	274	9.6.8.7	275	4.2.2.6 5.3	276	4.4.4.7.5	277	3.2
283	7.7.8	284	8.7	285	3	286	5	287	3.2

LE 5

MBER 3 IN PROBLEM 2

T.	S. 6 5.6.7.8.9	T.	S. 7 1.2.3.4.5 6.7.8.9	T.	S. 8 3.4.5.6.7.8	T.	S. 9 7.8.9	T.	S. 10 2.3.4.5
6	8.9.9.6a	7	2a	8	3.4a	9	9.8a	10	3a
16	6a	17	2a	18	4a	19	8	20	3a
26	6a	27	2a	28	4a	29	8	30	3
36	5.8.7.5 5.5.6a	37	3.2	38	3.3.3.3 3.3.3.4	39	7.8a	40	3
46	5.5.6a	47	2	48	4	49	7.7.7.7.7 7.7.9.7.8a	50	4.2.3
56	5.6	57	2	58	3.3.3.3.4	59	7.7.7.9.7 7.9.7.8	60	2.3
66	8.9.7.6			68	3.3.6.8 5.6.4	69	8	70	3
76	8.5.6	77	2	78	3.4	79	7.8	80	3
86	6	87	1.2	88	4	89	7.8	90	2.3
96	6	97	2						
98	5.6	99	2	100	3.3.6.4	101	7.8	102	3
108	7.8.9.5 8.6	109	3.4.5.1 2	110	3.3.3.3.4	111	7.8	112	2.2.3
118	5.5.6	119	3.4.2	120	3.4	121	7.8	122	2.3
128	7.8.9.6	129	3.4.5.2	130	3.4	131	7.8	132	2.4.5.2.3
138	7.8.5.8 9.6	139	1.2	140	3.4	141	8	142	2.3
148	6	149	3.4.5.6.7 8.9.5.5 6.7.8.2	150	3.4	151	7.7.7.8	152	2.3
158	8.9.5.5.6	159	2	160	3.4	161	7.8	162	2.3
168	5.6	169	2	170	3.4	171	7.8	172	3
178	5.6	179	1.2	180	3.4	181	7.8	182	2.3
188	5.6	189	2	190	4	191	7.8	192	3
198	5.6	199	2	200	4	201	7.8	202	4.5.5.5 2.3
203	5.6								
208	5.6a	209	1.2	210	8.8.3.3 3.4	211	8a	212	2.3
218	7.8.8.6a	219	2	220	4	221	8	222	4.5.4.2.3
228	8.9.6	229	2	230	4	231	7.8	232	4.5.2.3
238	8.9.7.6	239	3.4.5.6 7.2	240	3.3.3.3.4	241	8	242	3
248	5.6	249	2	250	3.3.3.6.5.4	251	7.7.8a	252	3
258	5.6	259	2	260	3.3.3.7.8 7.5.3.3.4	261	7.7.8	262	3
268	7.6	269	1.2	270	7.3.8.6.8 6.3.3.4	271	7.8	272	3
278	8.7.6	279	3.2	280	8.7.6.4	281	7.8	282	3
288	9.8.7.6	289	3.1.2	290	4	291	7.7.8	292	4.3

TABLE 5—

Results for Crow Number

T.	S. 1 7.**8**.9	T.	S. 2 6.**7**.8.9	T.	S. 3 2.**3**.4.5.6.7	T.	S. 4 4.**5**.6.7	T.	S. 5 1.**2**.3.4.5.6
293	8	294	8.7	295	3	296	4.4.5	297	3.2
303	8	304	6.7	305	5.4.3	306	4.4.7.6.4 6.4.6.7.5	307	3.1.2
313	7.7.8	314	7	315	3	316	4.4.4.6.5	317	3.2
323	8	324	8.7	325	3	326	4.5	327	4.3.1.2
333	7.8	334	8.7	335	4.3	336	6.7.4.5	337	1.2
343	8	344	6.7	345	2.2.3	346	7.5	347	2
353	8	354	8.7	355	3				
361	8	362	8.7	363	2.2.3	364	6.5	365	3.2
371	8	372	7	373	4.3	374	7.6.5	375	3.2
381	7.8	382	7	383	4.2.2.3	384	4.4.4.5	385	2
391	8	392	8.7	393	3	394	6.5	395	2
401	8	402	8.7	403	3	404	6.5	405	2
411	8	412	6.7	413	2.2.3	414	5	415	3.2
421	7.8	422	6.7	423	3	424	4.4.4.5	425	2
431	8	432	8.7	433	3	434	4.5	435	1.2
441	8	442	8.7	443	4.3	444	6.5	445	2
451	8	452	7	453	2.4.3	454	4.5	455	2
461	8	462	6.7	463	2.4.3	464	7.6.5	465	2
471	8	472	8.7	473	2.4.3	474	7.6.4.5	475	5.4.5.6 5.5.4.2
481	7.8	482	8.7	483	3	484	7.4.5	485	2
491	8	492	9.8.7a	493	2.3	494	6.4.5	495	4.3.2

Results for Crow Nu

6	9.9.8a	7	9.7a	8	4.3a	9	5a	10	2a
16	8a	17	7a	18	3	19	5	20	5.6.6.6 2a
26	7.8	27	6.7	28	4.6.7.3	29	4.6.7.7 4.4.5a	30	4.5.3.2a
36	8	37	8.9.7	38	4.6.2.7 7.3a	39	7.5a	40	2a
46	7.9.8	47	7	48	4.6.5.7.2 7.7.2.2.3a	49	5	50	3.6.2
56	8	57	7	58	2.7.2.6.3a	59	4.4.4.7.5	60	2
66	9.8	67	7a	68	2.3	69	7.4.7.6.5a	70	3.2
76	7.7.8	77	6.8.6.8 9.8.7a	78	2.7.2.6 2.3	79	4.7.5a	80	2a
86	8	87	6.8.7	88	2.7.7.3	89	5	90	6.1.4.6.2

Continued

3 IN PROBLEM 2—*Continued*

T.	S. 6 5.**6**.7.8.9	T.	S. 7 1.**2**.3.4.5. 6.7.8.9	T.	S. 8 3.**4**.5.6.7.8.	T.	S. 9 7.**8**.9	T.	S. 10 2.**3**.4.5
298	8.7.6	299	2	300	4	301	7.7.8	302	4.3
308	5.6	309	8.7.5.3.2	310	7.6.4	311	8	312	5.3
318	7.6	319	2	320	8.7.6.5.4	321	7.8	322	4.3
328	7.6	329	4.3.1.2	3 0	3.4	331	8	332	5.5.3
338	8.6	339	1.2	340	3.3.3.4	341	7.7.7.8	342	4.3
348	7.6	349	4.3.4.3 1 1 2	350	3.3.3.4	351	7.8	352	5.4.3
356	7.8.7.6	357	2	358	8.7.5.4	359	8	360	3
366	7.6	367	4.3.2	368	5.4	369	8	370	5.4.3
376	8.7.6	377	3.2	378	6.5.4	379	8	380	2.3
386	8.7.6	387	2	388	7.6.5.4	389	9.8	390	5.4.3
396	8.7.6	397	2	398	5.4	399	7.7.8	400	2.2.2.3
406	7.6	407	2	408	8.7.6.5.4	409	7.7.8	410	3
416	7.6	417	4.3.2	418	3.3.4	419	8	420	4.3
426	5.5.6	427	3.2	428	3.4	429	8	430	3
436	8.6	437	3.2	438	3.4	439	7.8	440	2.4.4.3
446	6	447	2	448	6.5.4	449	8	450	2.2.4.3
456	8.7.6	457	1.2	458	6.7.5.4	459	7.8	460	2.2.3
466	7.6	467	3.2	468	3.5.4	469	7.8	470	2.3
476	8.7.9.8 7.8.8.7 8.8.6	477	2	478	4	479	8	480	5.4.3
486	8.7.8.5 7.7.9.8 8.6a	487	2	488	3.4a	489	7.8	490	2.2.3
496	9.8.8.8 5.6	497	2	498	3.3.4	499	9.9.8	500	3

MBER 4 IN PROBLEM 2

1	9.9.6a	2	2a	3	4a	4	7.8	5	5.5.2.2 5.5.3a
11	6a	12	3.2a	13	4a	14	8a	15	4.3a
21	6	22	5.6.6.8.2	23	4	24	7.8	25	4.5.5.2 2.3
31	5.9.5.9.8 5.9.5.6a	32	4.2	33	3.3.3.4a	34	8	35	5.5.2.5.2 5.5.2.3a
41	9.9.6a	42	1.6.2a	43	5.8.3.8 4a	44	7.9.7.8	45	4.3
51	5.9.8.6	52	3.2	53	4	54	8	55	4.5.3
61	9.5.7.9 7.9.8.7 9.5.8.6a	62	3.9.1.8 4.2	63	3.8.4	64	8	65	2.5.3a
71	9.8.6	72	9.1.6.3 7.2	73	3.8.8.4a	74	7.9.8	75	2.5.2.4.3
81	8.9.8.7 6	82	8.8.2	83	3.4	84	8	85	2.2.3a
91	8.8.6	92	8.4.7.1 8.9.1.4 8.4.2a	93	4	94	7.7.8	95	3

TABLE 5—
RESULTS FOR CROW NU

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.**8**.9	T.	6.**7**.8.9	T.	2.**3**.4.5.6.7	T.	4.**5**.6.7	T.	1.**2**.3.4.5.6
96	9.8	97	8.8.8.6 8.7a						
98	7.9.7.7 7.7.7.8	99	8.9.9.7	100	7.4.2.6 2.4.4.3a	101	7.7.6.4 4.5	102	2
108	7.9.8	109	6.9.8.7	110	3	111	4.7.4.4 6.4.6.5	112	2
118	8	119	6.8.7	120	2.6.4.6 2.7.3	121	4.6.4.5a	122	1.1.4.2
128	7.8	129	7	130	3	131	4.6.6.4 6.7.4.5	132	1.3.3.1 5.2
138	7.8	139	7	140	2.4.2.3	141	4.6.5	142	1.3.2
148	7.8	149	8.6.8.6 6.8.6.6.7	150	5.2.7.2 5.2.4.2.3	151	4.5a	152	2
158	8	159	8.8.8.7	160	3	161	4.4.5	162	2
168	7.7.8	169	6.8.8.7	170	2.3	171	5	172	1.6.4.2
178	8	179	7	180	5.2.2.6 4.3	181	6.4.4.7 4.6.6.5	182	2
188	8	189	7	190	4.2.3	191	5	192	1.3.2
198	7.8	199	6.7	200	4.2.3	201	6.5	202	3.1.4.2
208	8	209	7	210	5.3	211	4.4.7.6 4.5a	212	2
218	8	219	8.6.6.9 7	220	4.3	221	7.5	222	2
228	7.8	229	6.7	230	2.4.3	231	4.4.4.7.4 4.6.4.4.5	232	1.3.2
238	7.8	239	7	240	3	241	7.7.4.5	242	2
248	8	249	7	250	2.3	251	4.5	252	1.2
258	7.8	259	6.7	260	2.4.5.6.3	261	5	262	2
268	8	269	6.7	270	2.3	271	4.5	272	1.2
278	7.8	279	6.7	280	2.3	281	4.5	282	1.3.1.2
288	7.9.7.8	289	6.7	290	2.3	291	5	292	1.2
298	7.8	299	6.7	300	2.4.2.3	301	4.5	302	1.3.5.3 4.1.2
308	7.8	309	7	310	2.3	311	6.7.5	312	1.2
318	8	319	8.8.7	320	6.7.4.2 2.3	321	4.5	322	1.3.4.2
328	8	329	7	330	2.4.2.3	331	5	332	6.3.4.1.2
338	8	339	8.8.6.7	340	2.2.3	341	5	342	3.2
348	8	349	7	350	2.3	351	4.5	352	1.2
356	8	357	6.7	358	7.5.2.3	359	7.4.5	360	6.3.4.1.2
366	8	367	8.7	368	2.4.6.7 4.5.3	369	5	370	3.4.5.2
376	7.8	377	5.7.8.6*	378	3	379	5	380	3.4.5.2
386	7.8	387	6.7	388	2.3	389	4.5	390	1.3.4.2

* Mistake, setting 5-9

Continued

MBER 4 IN PROBLEM 2

	S. 6		S. 7		S. 8		S. 9		S. 10
T.	5.**6**.7.8.9	T.	1.**2**.3.4.5 6.7.8.9	T.	3.**4**.5.6.7.8	T.	7.**8**.9	T.	2.**3**.4.5
103	6	104	8.2	105	4	106	9.8	107	3
113	9.7.8.7 9.8.6	114	3.8.7.4 6.2	115	3.6.3.3 3.4	116	8	117	2.3
123	5.9.7.6	124	2	125	3.6.8.6.4	126	7.7.7.7.. 7.8	127	3
133	5.8.8.6	134	1.3.8.7 8 6 7 2	135	3.5.3.7 3 6 4	136	7.9.7.7 7.8	137	2.2.3a
143	9.5.8.6	144	1.8.5.7 1.4.5.3.2	145	3.4	146	8	147	2.3
153	8.7.8.7 6	154	2	155	3.4	156	7.8	157	3
163	8.9.9.6	164	2	165	3.3.4	166	8	167	2.2.5.2.3
173	8.8.6	174	2	175	4	176	8	177	3
183	8.6	184	2	185	3.3.3.5.4	186	7.7.7.8	187	2.2.3
193	6	194	3.6.7.8 5.4.6.3 4 3 2	195	4	196	7.9.7.8	197	4.3
203	7.9.7.7 8.6	204	2	205	4	206	7.8	207	3
213	6	214	3.4.9.5 3.2	215	3.3.4	216	.78.	217	2.3
223	9.7.5.8 7.8.5.7.6	224	3.1.2	225	5.3.3.3.4	226	9.7.8	227	2.3
233	5.6	234	3.2	235	5.3.7.3.4	236	8	237	2.3
243	8.8.6	244	2	245	3.4a	246	8	247	2.3
253	6	254	3.4.3.2	255	4	256	8	257	3
263	7.8.7.8.7 7.5.8.7.6	264	2	265	4	266	8	267	2.2.2.3
273	5.6	274	3.4.2	275	8.7.7.3 5.7.4	276	7.8	277	2.3
283	5.6	284	3.4.2	285	5.6.4	286	7.8	287	4.2.3
293	5.6	294	2	295	3.4	296	7.8	297	2.4.2.3
303	8.6	304	1.2	305	5.7.6.4	306	7.8	307	3
313	5.7.8.6	314	1.3.4.5 6.7.5.2	315	7.6.7.6 7.4	316	9.8	317	2.3
323	8.7.5.6	324	5.6.2	325	5.4	326	8	327	2.3
333	6	334	1.2	335	8.6.3.4	336	7.8	337	2.3a
343	7.8.6	344	1.2	345	3.4	346	7.8	347	5.2.3
353	8.9.5.6	354	7.8.6.4 5.4.6.2	355	4				
361	8.7.8.5.6	362	2	363	4	364	9.7.8	365	4.2.3
374	5.7.8.9 5.5.6	372	2	373	4	371	7.8	375	3
381	5.6	382	2	383	3.4	384	7.8	385	2.3
391	5.6	392	1.3.2	393	3.4	394	7.8	395	2.3

TABLE 5—
RESULTS FOR CROW NUMBER

	S. 1		S. 2		S. 3		S. 4		S. 5
T.	7.**8**.9	T.	6.**7**.8.9	T.	2.**3**.4.5.6.7	T.	4.**5**.6.7	T.	1.**2**.3.4.5.6
396	8	397	6.7	398	4.4.5.6.3	399	4.5	400	1.3.4.5.2
406	7.8	407	8.6.7	408	2.3	409	4.5	410	2
416	7.8	417	7	418	2.4.5.2.4 2.4.2.4.3	419	5	420	1.2
426	7.9.7.8	427	6.7	428	2.3	429	4.6.4.5	430	2
436	8	437	6.7	438	3	439	5	440	6.2
446	8	447	6.8.8.6 8.6.7	448	2.2.4.5 6.2.4.2.3	449	4.5	450	2
456	8	457	6.7	458	2.4.2.3	459	4.5	460	3.2
466	8	467	8.6.8.8 6.7	468	3	469	4.5	470	3.4.3.2
476	8	477	7	478	2.4.5.2.3	479	4.5	480	1.2
486	8	487	8.7	488	2.4.2.3	489	7.6.5a	490	1.2
496	7.8	497	7	498	2.3	499	4.6.7.5	500	2

Concluded

4 in Problem 2—*Concluded*

	S. 6		S7		S. 8		S. 9		S. 10
T.	5 **6**.7.8.9	T.	1.**2**.3.4.5 6.7.8.9	T.	3.**4**.5.6.7.8	T.	7.**8**.9	T.	2.**3**.4.5
401	6	402	2	403	4	404	8	405	3
411	5.7.8.5.6	412	3.4.2	413	3.4	414	7.9.7.8	415	2.3
421	5.7.5.7 8.6	422	2	423	8.3.4	424	7.8	425	2.3
431	6	432	3.4.2	433	3.4	434	8	435	4.3
441	6	442	2	443	6.6.7.3.4	444	7.8	445	2.3
451	5.8.8.5 7.8.5.6a	452	2	453	3.4	454	8	455	2.3
461	5.6	462	3.1.2	463	3.4	464	7.7.8	465	2.3
471	5.7.5.6	472	2	473	3.4	474	8	475	2.4.2.3
481	8.6	482	2	483	3.4	484	7.8	485	3
491	5.6	492	2	493	3.4	494	7.8	495	2.3

TABLE 6

Daily Series and Averages, with Ratios of Correct to Incorrect First Choices

Problem 2

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 6	3	0	3				July 6	3	0	3			
" "	3	0	3				" "	3	0	3			
" "	2	0	2	0	8	0:8.	" "	2	0	2	0	8	0:8.
" 7	2	0	2				" 7	2	0	2			
" "	2	0	2				" "	2	0	2			
" "	2	1	1				" "	2	0	2			
" "	2	1	1	2	6	1:3.	" "	2	0	2	0	8	0:8.
" 8	1	0	1				" 8	1	0	1			
" "	3	1	2	1	3	1:3.	" "	3	2	1	2	2	1:1.
" 10	3	0	3				" 10	3	2	1			
" "	2	0	2				" "	2	0	2			
" "	3	0	3				" "	3	0	3			
" "	2	1	1	1	9	1:9.	" "	2	0	2	2	8	1:4.
" 11	3	0	3				" 11	3	0	3			
" "	2	0	2				" "	2	1	1			
" "	3	0	3				" "	3	1	2			
" "	2	1	1	1	9	1:9.	" "	2	0	2	2	8	1:4.
" 12	2	1	1				" 12	2	0	2			
" "	3	1	2				" "	3	0	3			
" "	3	2	1				" "	3	1	2			
" "	2	0	2	4	6	1:1.50	" "	2	1	1	2	8	1:4.
" 13	5	0	5				" 13	5	2	3			
" "	5	1	4				" "	5	3	2			
" "	5	2	3	3	12	1:4.	" "	5	1	4	6	9	1:1.5
" 14	5	1	4				" 14	5	0	5			
" "	3	0	3				" "	3	0	3			
" "	2	1	1				" "	2	0	2			
" "	5	2	3	4	11	1:2.75	" "	5	0	5	0	15	0:15
" 15	5	0	5				" 15	5	1	4			
" "	5	2	3				" "	5	1	4			
" "	5	1	4				" "	5	2	3			
" "	2	2	0	5	12	1:2.40	" "	2	0	2	4	13	1:3.25
" 16	5	2	3				" 16	5	1	4			
" "	5	0	5				" "	5	3	2			
" "	5	0	5				" "	5	2	3			
" "	5	2	3	4	16	1:4.	" "	5	1	4	7	13	1:1.85
" 17	5	0	5				" 17	5	1	4			
" "	5	2	3				" "	5	2	3			
" "	5	0	5				" "	5	2	3			
" "	5	2	3	4	16	1:4.	" "	5	0	5	5	15	1:3.
" 18	5	1	4				" 18	5	1	4			
" "	5	0	5				" "	5	1	4			
" "	5	1	4				" "	5	1	4			
" "	5	1	4	3	17	1:5.66	" "	5	2	3	5	15	1:3.
" 19	5	1	4				" 19	5	3	2			
" "	5	0	5				" "	5	2	3			
" "	5	2	3				" "	5	1	4			
" "	5	0	5	3	17	1:5.66	" "	5	4	1	10	10	1:1.
" 20	5	0	5				" 20	5	3	2			

TABLE 6—*Continued*

Daily Series and Averages, with Ratios of Correct to Incorrect First Choices

Problem 2—*Continued*

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 20	5	0	5				July 20	5	1	4			
“ “	5	3	2				“ “	5	3	2			
“ “	5	1	4	4	16	1:4.	“ “	5	2	3	9	11	1:1.22
“ 21	5	2	3				“ 21	5	0	5			
“ “	5	1	4				“ “	5	3	2			
“ “	5	0	5				“ “	5	3	2			
“ “	5	1	4	4	16	1:4.	“ “	5	1	4	7	13	1:1.85
“ 22	5	3	2				“ 22	5	2	3			
“ “	5	1	4				“ “	5	0	5			
“ “	5	2	3				“ “	5	0	5			
“ “	5	2	3	8	12	1:1.50	“ “	5	1	4	3	17	1:5.66
“ 23	5	2	3	2	3	1:1.05	“ 23	5	3	2	3	2	1: .66
“ 24	5	2	3				“ 24	5	2	3			
“ “	5	2	3				“ “	5	2	3			
“ “	5	1	4				“ “	5	4	1			
“ “	5	2	3	7	13	1:1.85	“ “	5	2	3	10	10	1:1.
“ 25	5	3	2				“ 25	5	3	2			
“ “	5	1	4				“ “	5	1	4			
“ “	5	0	5				“ “	5	0	5			
“ “	5	1	4	5	15	1:3.	“ “	5	0	5	4	16	1:4.
“ 26	5	2	3				“ 26	5	0	5			
“ “	5	1	4				“ “	5	1	4			
“ “	5	2	3				“ “	5	1	4			
“ “	5	2	3	7	13	1:1.85	“ “	5	0	5	2	18	1:9.
“ 27	5	1	4				“ 27	5	1	4			
“ “	5	1	4				“ “	5	1	4			
“ “	5	1	4				“ “	5	0	5			
“ “	5	1	4	4	16	1:4.	“ “	5	1	4	3	17	1:5.66
“ 28	5	2	3				“ 28	5	1	4			
“ “	5	1	4				“ “	5	3	2			
“ “	5	0	5	3	12	1:4.	“ “	5	1	4	5	10	1:2.
“ 30	5	0	5				“ 30	5	1	4			
“ “	5	2	3				“ “	5	0	5			
“ “	5	0	5				“ “	5	2	3			
“ “	3	1	2	3	15	1:5.	“ “	3	1	2	4	16	1:4.
“ 31	5	3	2				“ 31	5	1	4			
“ “	5	1	4				“ “	5	2	3			
“ “	5	1	4	5	10	1:2.	“ “	5	2	3	5	10	1:2.
Aug. 1	5	2	3				Aug. 1	5	3	2			
“ “	5	1	4				“ “	5	2	3			
“ “	5	2	3				“ “	5	1	4			
“ “	5	1	4	6	14	1:2.33	“ “	5	0	5	6	14	1:2.33
“ 2	5	3	2				“ 2	5	0	5			
“ “	5	1	4				“ “	5	1	4			
“ “	5	3	2	7	8	1:1.14	“ “	5	5	0	6	9	1:1.5
“ 3	5	2	3				“ 3	5	1	4			
“ “	5	2	3				“ “	5	0	5			
“ “	5	1	4	5	10	1:2.	“ “	5	2	3	3	12	1:4.
“ 4	5	2	3				“ 4	5	1	4			

TABLE 6—*Continued*

Daily Series and Averages, with Ratios of Correct to Incorrect First Choices

Problem 2

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug. 4	5	2	3				Aug. 4	5	1	4			
“ “	5	2	3				“ “	5	2	3			
“ “	5	0	5	6	14	1:2.33	“ “	5	3	2	7	13	1:1.85
“ 5	5	2	3				“ 5	5	2	3			
“ “	5	3	2				“ “	5	2	3			
“ “	5	3	2	8	7	1: .87	“ “	5	2	3	6	9	1:1.5
“ 6	5	0	5				“ 6	5	1	4			
“ “	5	2	3				“ “	5	0	5			
“ “	5	0	5				“ “	5	2	3			
“ “	5	1	4	3	17	1:5.66	“ “	5	1	4	4	16	1:4.
“ 7	5	2	3				“ 7	5	2	3			
“ “	5	2	3				“ “	5	2	3			
“ “	5	1	4				“ “	5	1	4			
“ “	5	1	4	6	14	1:2.33	“ “	5	1	4	6	14	1:2.33
“ 8	5	2	3	2	3	1:1.5	“ 8	5	2	3	2	3	1:1.5

The summary of choices given in table 5 is chiefly valuable as a means of detecting reactive tendencies. But it also indicates that neither crow succeeded in solving the problem. We had supposed, from our previous experience, that within two or three weeks the crows would be choosing the second compartment at the left with ease, but as a matter of fact, with the appearance and disappearance of the more or less unsatisfactory reactive tendencies apparent in table 5, they continued their work over a period of several weeks without mastering the situation. It seemed utterly useless to continue the experiment with this problem beyond the five hundredth trial. Had there been any consistent improvement, even although extremely slow, we should have felt justified in continuing the training.

The presentation of results in table 6 is of interest primarily because the reader can from it see the fluctuation in the measure of success during the long continued period of training. We have presented in this table for each bird the number of correct and incorrect first choices by series of trials under each date. Following the results appear the ratios of successes to failures for each day.

In table 7 the ratios of correct to incorrect first choices are given for the trials by groups of twenty-five, in order that the influence of "good" and "bad" days may be fairly distributed.

TABLE 7

RATIOS OF CORRECT TO INCORRECT FIRST CHOICES IN PROBLEM 2 BY GROUPS OF TWENTY-FIVE

Trials	Crow No. 3	Crow No. 4
1- 25	1: 7.30	1: 5.25
26- 50	1: 3.16	1: 5.25
51- 75	1: 4.00	1: 3.16
76-102	1: 2.00	1: 4.40
103-127	1: 5.25	1: 1.77
128-152	1: 5.25	1: 1.40
153-177	1: 5.25	1: 1.08
178-202	1: 3.16	1: 1.77
203-227	1: 3.16	1: 1.77
228-252	1: 1.50	1: 2.12
253-277	1: 2.57	1: 1.50
278-302	1: 1.12	1:11.50
303-327	1: 3.16	1: 5.25
328-352	1: 7.33	1: 2.57
353-375	1: 1.87	1: 1.55
376-400	1: 2.12	1: 5.25
401-425	1: 1.15	1: 1.77
426-450	1: 1.77	1: 1.50
451-475	1: 3.16	1: 3.16
476-500	1: 2.12	1: 2.12

The probability of a correct choice in this experiment, supposing that chance alone is involved[5], is one to four. At the beginning of the experiment, it is noted (table 7) that the ratio for number 3 was 1 to 7.30; that for number 4, 1 to 5.25. For neither bird does the ratio fall as low as 1 to 1 at any time during the training. The nearest approach to this measure of success was made by crow number 4 in the trials 153 to 177, for which the ratio was 1 to 1.08.

Further, it is to be noted that neither crow shows a steady increase in the number of correct choices. There is, instead, for each, an increase up to a certain point, then a sudden decrease, followed by a more or less rapid increase. Number 3 exhibits three well marked improvement waves. Beginning with the ratio 1 to 7.3, there is fairly constant improvement until the ratio stands 1 to 2. Then a backsliding occurs which, for the next twenty-five trials results in a ratio of 1 to 5.25. Slowly the bird improves again, achieving, after about three hundred trials, a ratio of 1 to 1.12. But this, after fifty additional trials, is replaced by a ratio of 1 to 7.33. Immediately thereafter, rapid improvement sets in, and shortly a ratio of 1 to 1.15 results.

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[5] Of course the previous work on Problem 1 influenced the birds very markedly in their early trials.

The same in general holds for number 4. At the end of fifty trials, its ratio is 1 to 5.25. After one hundred and seventy-seven trials, it is 1 to 1.08. Then the number of correct first choices slowly decreases until finally, at about the three hundredth trial, the ratio is 1 to 11.50. There follows, during the next seventy-five trials, improvement which results in the ratio of 1 to 1.55, which, in turn, is immediately followed by the ratio of 1 to 5.25.

These fluctuations in the ratio of right to wrong first choices are indicative of the appearance, "trying out," and disappearance of more or less satisfactory reactive tendencies. The fact that neither bird achieved a ratio of 1 to 0 indicates that no reactive tendency appeared which was wholly satisfactory, or in other words, led to the complete solution of the problem. These various reactive tendencies we should describe, in the case of a human subject, as the "trying out" of ideas, but it is unnecessary for us to have recourse to this mode of psychological description in the case of the crows. They may or may not have had ideas corresponding to those which would have existed in the ordinary human subject. In any event, their behavior is strikingly similar to that of the human subject of a low level of intelligence.

ANALYSIS OF THE REACTIONS OF CROW NUMBER 3, ♂

An analysis of the data of table 5 renders these fluctuations of the ratio of correct to incorrect first choices at once intelligible and deeply significant. We shall attempt an analytical examination of the results for each subject in order to bring the several reactive types and tendencies into prominence.

For crow number 3 the first thirty or forty trials in problem 2 in large measure destroyed the subject's well formed habit of choosing first, as a result of previous training in problem 1, the first compartment at the right. The bird then began to choose very frequently the first compartment at the left and to distribute the remainder of its choices among the other compartments, until the right one happened to be chosen. From the beginning of work on this problem, the persistency of number 3, as also of number 4, in reentering the same compartment was surprising. Examples of this are trials three, twenty-one, twenty-four, thirty-eight, forty-nine, fifty-nine, sixty-four, and so on, for number 3.

By the time number 3 had been given one hundred trials, a habit of always going to the first compartment at the left, and after receiving the punishment of confinement in the compartment, entering the one next in order, had become fairly well fixed. For some of the settings, this habit developed earlier than for others. For instance, in settings Nos. 1 and 9, 7-9, this particular reactive tendency appeared first in the thirty-ninth trial, again in the sixty-first, and in the seventy-ninth, when it seems to have been accepted as the most satisfactory method of reacting, and appears as a habit for almost two hundred trials.

With setting No. 2, 6-9, this same reactive tendency appears definitely at about the ninety-second trial; for setting No. 3, 2-7, at about the one hundred and fifty-fifth; for No. 4, 4-7, at the one hundred and twenty-sixth; for No. 5, 1-6, at the one hundred and twenty-sixth; for No. 6, 5-9, at the one hundred and sixty-eighth; for No. 8, 3-8, at the one hundred and twentieth; for No. 10, 2-5, at the one hundred and forty-second; but for No. 7, 1-9, neither this tendency nor any other became well established during the five hundred trials.

In the fifty trials, one hundred and fifty-one to two hundred inclusive, the habit of entering the first compartment at the left, and next the adjacent one, which of course was also the correct one, appeared thirty-eight times. In ten of these fifty trials, the right compartment was entered immediately, and in the remaining two trials, the compartment first entered was the second from the right instead of the second from the left end of the group.

From trials two hundred and forty to two hundred and fifty, a new reactive tendency began to appear. This shortly replaced the one just described. Previously, number 3, when it came out of the first compartment at the left, turned sharply to its left and entered the second compartment, the right one, but now instead of turning to its left, it began to turn to its right, with the result that it faced the compartment which it had just left. Formerly, it had always met with reward when, after coming out of the first compartment and turning sharply it entered the one directly in front of it, but now it met, instead, with punishment. Nevertheless, it persisted in reentering the wrong compartment, and in trial two hundred and fifty-three it was punished thirteen times for entering compartment 7. It was then aided in finding the right compartment. In this instance, even after the door of the wrong compartment which had been so often reentered had been closed and the door of the right compartment left open beside it, the bird stood for some seconds before the wrong door, cawing and apparently eager to enter.

Naturally the tendency to turn to its right instead of to its left greatly diminished the number of correct first choices by number 3, and completely obliterated the old reactive tendency. Shortly, the number of reentrances diminished to two or three, and the bird began to enter the second compartment from the left, even although it were not facing it after it turned about. This peculiar behavior continued for only a short time, and was followed by a tendency to enter first a compartment near the right end of the series. On escaping from this, it would turn to its right and enter the compartment directly in front of it. Repetition of this performance of course soon brought the bird to the right compartment. In trial after trial, number 3 would enter the first compartment at the right and then work back, compartment by compartment, until it reached the second from the left. Examples of this behavior are trials two hundred and eighty, two hundred and eighty-eight, three hundred and nine, three hundred and twenty, and so on.

After the thorough testing of the reactive tendency just described, no additional habit became well established, but the crow shifted from one method to another. The one most often used was that of entering the third from the left, and on leaving this, turning to the right and entering the compartment before it, which was of course the right one. This method is exhibited for setting No. 2, 6-9, after the two hundred and eighty-fourth trial; for setting No. 5, 1-6, after trial two hundred and seventy-seven; for setting No. 6, 5-9, after trial three hundred and eighteen; and for setting No. 10, 2-5, after trial two hundred and ninety-two. For the other settings, it appeared less frequently.

To complete our comments on the behavior of number 3, we may say that beginning with trial four hundred and sixty-one, the period of punishment was increased from thirty seconds to sixty seconds, since it seemed possible that the crow might improve under this condition. But the punishment was over-severe, and after only a few trials, number 3, as also number 4, began to work very badly indeed, It would move about constantly and excitedly while confined in a compartment, and when the door was opened would rush out and immediately enter another compartment without pause. This random and excited choosing naturally yielded few successes, and by the time forty trials had been given, number 3 was very hesitant about entering any of the compartments and had returned to an earlier habit of wandering about the reaction chamber. When the time of punishment was reduced to fifteen seconds, he very quickly resumed his former method of reacting, and worked quite as assiduously as ever, and with as small a measure of success.

ANALYSIS OF THE REACTIONS OF CROW NUMBER 4,♀

The behavior of number 4 in problem 2 differs in some respects from that of number 3 and is worthy of brief description. The first fifty trials served to break up the habit of choosing the first compartment at the right. Thereupon, her attention shifted to the opposite end of the series. But this was not so definite as in the case of number 3. Number 4 often went to the first compartment at the left and then to the one next to it, thus requiring but two choices in order to get the right compartment. This tendency became fixed only after two hundred and fifty trials, and even then it was not so definite as for number 3.

It is indicative of the temperamental differences in the two subjects that number 4 should have required assistance in almost twice as many of the first two hundred trials as did number 3. Significant also is the fact that until very late in her training she was not nearly as systematic in her choices as was he. She tended rather more frequently to the compartments near the middle instead of those at the ends, and chose in no definite or predictable way. This naturally resulted in a much larger number of choices before the right compartment was reached, than in the case of number 3. Discouragement was proportional to the number of mistakes. But at the same time, number 4, just because of the lack of a definite inadequate reactive tendency, happened upon the right compartment more frequently than did number 3. For her, therefore, the ratio of correct to incorrect choices is more favorable than for him. This is true up to about the three hundredth trial, when it appears that number 4 for some reason became more systematic in her work, going most frequently to the first compartment at the left and then to the second. This habit, which had appeared also in the behavior of number 3, had by this time been replaced, and as a result he was more often choosing correctly than she. Number 4 continued to exhibit this reactive tendency rather insistently throughout the remaining trials.

We must conclude from this analysis of the data of table 5 that both crows, in the five hundred trials with problem 2 which were given them, tried and found inadequate all of the reactive tendencies which were immediately available. Toward the end of the experiment, it was evident, especially in the case of number 3, that the bird's only resource was to return to some one of the methods previously employed. Strange as it may seem to the human subject, and especially to those human beings who have a high estimate of the intelligence and originality of the crow, these individuals proved entirely incapable of learning to enter directly the second compartment from the left in a series of compartments.

Doubtless, many readers will object that longer training would almost certainly have enabled our subjects to solve this problem. We cannot deny this possibility, but we must insist that all of the indications of our results are against it, for ordinarily the adequate solution of such a problem as this by an animal of intelligence far lower than the human is achieved by slow improvement. We are of the opinion that the crow is incapable of perceiving and properly reacting to the relation of second from the left, and we do not hesitate to admit that we were very much surprised by this outcome of our experiments, as we had fully expected our subjects, and especially crow number 3, to deal successfully with much more difficult problems than this one.

This opinion rests not solely upon the fact that no steady and consistent improvement occurred as the result of five hundred trials distributed over a period of thirty-two days, but also upon the observation that in the case of the setting 7-9, which appeared twice in the series, as Nos. 1 and 9, and was therefore presented to each crow one hundred times instead of fifty times, the successes were surprisingly few. In the first presentation of this setting, number 1 in the series, for both of the crows there is marked increase in the number of correct first choices between the beginning and the end of the training. But for the second presentation, number 9 in the series, this is not the case. Crow number 3, in the first ten presentations of this setting, as number 9 in the series, chose correctly only twice, and in the last ten, only four times, while crow number 4 chose correctly in the first ten, four times, and in the last ten, four times. Even without experience they should have chosen correctly twice in ten trials.

This is an easy setting and it is surprising indeed that the crows should not have succeeded in reacting correctly in the latter part of the training. Doubtless their failure is due to the confusing effect of the diverse settings.

We feel that our analysis and discussion of results is inadequate, but the report is already overlong because of the necessarily lengthy description of apparatus and experimental procedure, and we may add only a brief summary.

SUMMARY

1. Two crows, No. 3, a male, and No. 4, a female, about three months old, were presented with two of the simplest types of standard problem in the Yerkes multiple choice apparatus. These problems were: (1) selection of the compartment first at the right in a series; (2) selection of the compartment second at the left; and (3) the other form of problem 1, the selection, namely, of the compartment first at the left.

2. Of these three problems both birds succeeded in solving perfectly, in from fifty to one hundred trials, the first and the last. The second problem they failed to solve in five hundred trials.

3. Various types of reaction and reactive tendencies appeared during the work on problem 2. Examples of these are: (1) to go to the first compartment at the right because of training in problem 1; (2) to go to the first at the left, and then to the

next in order ;(3) to reenter the compartment first chosen and then to choose the second from the left of the series; (4) to enter a compartment at or near the right end of the series and on emerging to turn to the right and enter the one directly in front, and so on until the right compartment is entered.

4. Since no one of these types of reaction is satisfactory, the birds shifted from one to another, trying them for varying periods.

5. The male was more tame, bold, and aggressive than the female. Consequently, he made the better showing in the experiments.

The multiple choice method, with four standardized problems, has been employed with pigs, rats, and ring-doves, as well as with crows; and, among human subjects, with normal and defective children and adults and with dementia praecox patients. The results will appear in a series of papers of which this is the first.

COLOR BLINDNESS OF CATS

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FOUR FIGURES

INTRODUCTION

The term "color-blindness" is used in the title of this paper, not because color-vision should be denied of an animal as a result of a single investigation, no matter how carefully it may have been conducted, but because the results of our experiments certainly make the term "color-blindness" a less presumptuous one than "color-vision" when applied to these animals.

The experiments were carried on in diffuse daylight and during the same hours each day, so that the results apply to the light-adapted eye of the cat. The fact that every discrimination and confusion made by one cat were also made by another, "the follower," would indicate that the conditions were not variable enough to cause a difference between the responses of the two animals. This agreement would indicate further that our results represent general characters of feline vision rather than individual peculiarities.

The investigation was begun in February 1911 and continued until June 1913. Thus it required twenty-eight months to test the animals with the large number of colors and grays which we employed.

We are indebted to Professor Lawrence W. Cole, under whose supervision the work was done, for invaluable advice and encouragement as well as for the suggestion of the problem. We also wish to acknowledge our obligation to Miss Mary E. Lakenan, who did some preliminary work toward devising a method suited to the animals to be tested.

Among the mammals, the vision of mice, rats, rabbits, squirrels, dogs, cats, raccoons, and monkeys has been investigated. Only one cat was included but Colvin and Burford[1] found it equal in color-vision to the dogs and the squirrel which they tested. They concluded that "The tests seem to show a surprising fineness of color-discrimination among the animals tested".

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[1] Colvin, S. S. and Burford, C. C. The color-perception of three dogs, a cat, and a squirrel. *Psych. Rev. Monog. Supp.*, vol. 11, Nov., 1909, pp. 1-49.

The chief results of recent experiments on mammalian color-vision have been the refinements in methods achieved through comparison and keen criticism. We cannot here deal with the technique of experiments other than our own, but the following generalizations will indicate the direction which the improvement in methods has taken.

1. So great a difference exists between the results obtained from the light-adapted and the dark-adapted eye that those secured under the one condition give us almost no clue to those which may result from the other.

2. All secondary criteria must be rigidly excluded. Differences of form, depth, size, texture, and especially differences of brightness between the lights or test papers used are especially to be guarded against.[2]

3. Much stress has been placed on the need of "natural conditions" in testing an animal's vision. It is reasonable to suppose that artificial conditions, like testing the animals in a dark room, or by artificial light, might lead to false views of his visual powers.

4. The stimulus color may be (a) reflected light from colored objects, usually papers, or (b) transmitted light which may be filtered through colored glasses or colored fluids, or isolated from certain spectra. The first method is used to test the light-adapted eye, or daylight vision, while the second is used chiefly to test the dark-adapted eye, or twilight vision.

The second class of methods strives to secure different hues of pure or homogeneous light. Both plans seek to find pairs of colors, or colors and grays which will be confused by the animal, assuming that the intensities of the colors for the animal and for human vision may not be the same.

Many criticisms of the use of colored papers for testing vision have been made. Their surfaces are said to differ greatly owing to accidents of manufacture. It is said to be difficult to bend them around glasses. They do not reflect homogeneous light but overlapping bands, etc. etc. None of these criticisms, however, applies very clearly to pairs of colors which are *confused* by the animal. Rather they are meant to point out all of the "secondary criteria" by which the animal *may discriminate* the colored papers. When an animal has confused a color and a gray for some six hundred trials, though quite accustomed to making discriminations, it seems fair evidence that he is unable to discriminate them by secondary criteria, nor even by the primary one, and that they appear the same to him.

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[2] See Yerkes, The Dancing Mouse, pp. 91-92, and 151.

After a confusion had been made between two colors (or a color and a gray) it seemed desirable to define it accurately for the sake of reproducing it at will. This we have done by stating its "flicker equivalent," a term used by Polimanti[3] to avoid any theoretical implication. A comparison of the equivalents we obtained with those of Cole and Long[4] shows that the flicker test reveals any fading of a colored paper and that the Bradley papers, except the violet hues, remain constant for a long time, if kept in darkness when not in use. In determining the flicker equivalents of the colored papers the decision was invariably based on inspection by two observers, and each color was tested with the gray just darker and just lighter than that with which it gave the minimum of flicker, a process which Ives[5] aptly calls "stepping off in various directions." Hence the decision in each case was the result of the comparison of at least three amounts of flicker. The method of determination was that described by Cole and Long (pp. 660-664). When a color gives a slight amount of flicker with each of two consecutive grays it is designated as between the two by giving the numbers of both. Thus Gray 1-2 means a gray which gives a slight amount of flicker when rotated with Hering Gray 1 or Hering Gray 2, a large amount when rotated with Gray 3, but which gives no flicker with those papers which in turn give very slight amounts with Grays both 1 and 2. These intermediate flicker equivalents occur because there are but fifty gray papers with which to test ninety or more colored papers. Cole and Long met this condition by establishing three amounts of flicker between each pair of grays. (p. 662). As we cannot adapt that device to the tables to be given in this paper, we shall designate a few of the colors by the numbers of both of the adjacent grays.

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[3] Polimanti, O. Ueber die sogennante Flimmer-Photometrie. *Zeitschrift fuer Psychologie*, vol. 19, 1899, S. 263ff.

[4] Cole, L. W. and Long, F. M. Visual discrimination of Raccoons. *Jour. of Comp. Neur. and Psych.*, vol. 19, 1909, p. 657ff.

[5] Ives, H. E. Photometry of lights of different colors. *Phil. Mag.*, 1912, p. 858.

In our search for grays which would be confused with the stimulus color we were finally driven to make tests with a number of gray cambrics. These also can be accurately defined by their flicker equivalents, and by no other method which we can discover. Of course the cambrics were used in both double and triple thicknesses, to prevent their transmitting light, both in experiments with the cats and in ascertaining their flicker values.

Quite aside from disputes as to the cause of the phenomena of flicker or what it measures, it makes colored and gray surfaces comparable, accurately definable and hence reproducible. It permits any series of colors or grays to be interpolated with any other since rotation obliterates differences of texture. It is accurate because very slight amounts of flicker are readily perceptible. Consequently we regard it as necessary to define our colors by their flicker equivalents, though our work is exploratory and qualitative.

While we have used flicker values merely for the sake of definition, Ives (p. 852) says of flicker photometry:

1. "It surpasses all other photometric methods in sensibility and reproducibility in the presence of color difference."
2. "It agrees at high illuminations with the equality of brightness method, when the latter is freed from the psychological uncertainties inherent in its use."
3. It measures at high illuminations what may fairly be termed the true brightness."
4. Brightnesses measuring equal to the same measure equal to each other and the sum of the measurements of the parts is equal to the measurement of the whole."

While these conclusions are drawn for the conditions of Ives's experiments they at least do not diminish the probability that our own flicker equivalents are at least rough approximations to measurements of brightness, though we have used them for the purpose of defining our papers.

In the following tables and description we shall use the initials F. E. for flicker equivalent and except where otherwise stated we shall not regard it as a measure of any property of the colored or gray papers but merely as the most accurate method of identifying any one of them.

We began this investigation with the intention of studying the vision of the cat under both daylight and twilight illumination, thus planning to use both reflected and transmitted light. Some work was done on the dark-adapted eye with filtered light, and we hope to carry the investigation further. At present we can report only the results obtained under light adaptation and by the use of colored papers. The enormous variation in the size of the cats' pupils suggested the possibility that this is a device which keeps the retina always in twilight. But as yellow, apparently, is the brightest of the colors for the cat as well as for the human eye, under light adaptation, the possibility of the animal's possessing twilight vision alone is very doubtful.

Three female and six male cats were tested. There is abundant evidence that these cats represented entirely non-related strains. Each cat was given from thirty to one hundred twenty trials a day according to his degree of hunger. One cat was given fifteen thousand trials and the trials of all the cats number over one hundred thousand.

By taking care not to over-feed the animals they were kept (with one exception) in excellent health throughout the progress of the experiments. The cats often purred during the experiments, which perhaps indicates that the conditions were not seriously "artificial".

The colors were presented in pairs and each cat was given preliminary training with easily discriminable pairs until his selection of the "food color" or stimulus color was rapid and accurate. Since most of the pairs were readily discriminated no cat reached a "confusion area" until he had been thoroughly trained in the experiment. With one animal we would begin at the red end of the Bradley series. With the other, which was to confirm or refute the results gained with the first, we began at the violet end, and so on. Since red is known to have a low stimulating power in the case of mice, rabbits, etc., we began our experiments with yellow and blue as stimulus colors.

From the beginning our object was to search systematically for confusions between a gray and a color, or between two colors, without any preconceived theory as to where, in the series, such a confusion might occur, or even whether it might occur at all. During hundreds of our first trials no confusions did occur, for we used Hering grays paired with Bradley colors. Discrimination was prompt and apparently this was due to the difference in texture between the two types of paper, for we later found numerous confusions. As shown by the cats' responses, pieces of ordinary cambric may be found which are so near the texture of the Bradley papers (when both are placed behind clear glass) that they are not discriminable by the animals. This is not true of Hering grays with Bradley papers. In such pairs we think that the Hering papers are not suitable for the study of any but the most defective eyes, because of their granular texture. This opinion is based on tests of the whole series of Hering grays with three cats. Miss Washburn[6] obtained admirable results with Hering grays and Bradley colors in investigating the vision of the rabbit. But as the rabbit has medullated nerve fibres passing in front of the retina[7] we should imagine that that defect would make the rabbit's eye very poor for the discrimination of textures.

In order to avoid awkward circumlocutions we have used such subjective terms as "confusion," "discrimination" (both "easy" and "difficult") etc. The reader will observe, however, that in every case these terms signify a definite objective condition, measured by the responses of the animals.

APPARATUS AND METHOD

The apparatus was that used by Cole and Long (p. 667). It was painted dead black and had black partitions between the glass holders. These partitions were necessary because in our experiments the colored papers were placed within the glasses. This permitted the possibility of a certain amount of reflection from a glass to the one beside it in case no black partitions were interposed. Since one cat seemed to make the choice of a glass by the position of the thumb-buttons, which fasten the levers, the buttons were concealed by shields of black cardboard cut for the purpose. The glasses mentioned were the feeding vessels and were ordinary jelly glasses, selected from a large assortment for clearness, freedom from flaws, and uniformity of size and shape. By means of the levers these glasses were clamped with their tops pressed closely against the under surface of the topboard, so that the animal must select the glass at which he pulled by its outside appearance only and without being able either to reach into it or look into it until he had pulled it down.

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[6] Washburn, M. F. and Abbot, Edwina. Experiments on the brightness value of the red for the light-adapted eye of the rabbit. *Jour. Animal Behavior*, vol. 2, 1912.

[7] Howell, W. H. Physiology, 1908, p. 319.

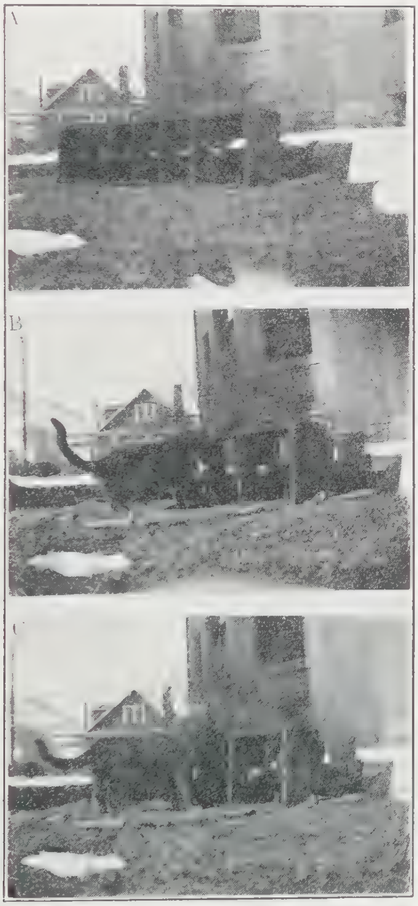

FIGURE 1. A. Apparatus for displaying the glasses which were tinted with colored papers. B. Cat depressing lever to release food-glass. C. Cat looking over the top of the apparatus in order to discriminate by the positions of the thumb-buttons.

By means of the thumb-buttons at the rear of the apparatus every glass except one was locked against the topboard. A pull at the glass which was on the free lever depressed its short arm and exposed the top of the glass so that the cat could reach into it or tip it over and secure the food which it contained. This glass will be referred to as the "food-glass" or the stimulus glass. The others will be called "confusion glasses," or confusion colors. The latter term will often include a gray where the context or the heading of the table has already specified a gray. From the front of the apparatus the colored papers, which lined the glasses made the only visible difference between them.

As already stated, the Bradley colored papers were used, and one blue cambric. After failing to find any confusions of the stimulus color with Hering grays we tried the cats with any gray we could obtain. The justification for this procedure is that we finally found a gray which two cats confused with each of the food-colors during at least six hundred trials. Only after this had been done was its flicker equivalent determined for the purpose of describing it.

At the beginning of the tests of each cat each day several glasses were cleaned and fitted with the necessary papers. The experimenter then took a position at one side and in front of the apparatus. Two glasses were displayed on adjacent levers and the cat was allowed to approach the apparatus from in front. If he went directly to the food-glass and drew it down, the choice was recorded as correct. But if he touched the confusion-glass ever so lightly, either with paws or head, the choice was called incorrect. After making his choice, the cat was allowed to discover the food-glass and secure food, but no record was made of this act. The cat was then placed where he could not see the levers while the glasses were being changed about. After this precaution, he was started again from his former position.

Thirty consecutive choices were considered a "series" and twenty-four or more right choices were required in a series before it was recorded as discrimination. If a cat consistently failed to discriminate two colors for twenty such series (six hundred trials), the result was recorded as complete confusion. In some instances an animal failed to make a discrimination for eight or more series, but showed by the low percentage of errors in each series that it might learn to discriminate and eventually did choose correctly twenty-four times in each of two consecutive series, thus making a discrimination. These were called "difficult discriminations." They doubtless indicate that the cats had to learn to discriminate what was for them close to a match. It was not difficult to identify a complete confusion, for the number of errors in each series almost invariably was twelve or more, while for six hundred tests the errors were not far from fifty per cent. On the other hand, the series in a difficult discrimination would contain a smaller proportion of errors, but it was here that greatest care was required.

Let us summarize the precautions taken to guard against discrimination by criteria other than the hue or intensity of the paper. The glasses were kept scrupulously clean so as to show no marks of any kind on their outer surface. They were all of clear glass without flaws. All of the papers were cut by a single pattern and placed in the glasses so that the overlapping edges were on the opposite side from the cat and hence invisible to him. Finally, circular disks of pasteboard were forced down within the cylinders of paper so that they held the paper closely pressed against the inner surface of the tumblers, and thus did away with wrinkles or apparent differences of depth. Several observers reported that the tumblers when thus prepared appeared to be made of colored glass. The apparatus prevented looking into the glasses before making a choice, and the experimenter put the glasses in position and made all changes while the cats were where they could not see the apparatus.

To prevent choice by position, the glasses were presented in random order, (one on the right, then on the left, then on the left again and so on), and they were changed in their positions along the apparatus. Whenever a cat formed the habit of choosing the glass on the left (or the right), the food-glass was placed no the other side until the habit was broken up.

To eliminate odor as a factor in discrimination, equal amounts of food were placed in the food-glass and the confusion glass; clean glasses and clean papers promptly replaced any that had become soiled, and the papers were exchanged within the two glasses so that the confusion-paper replaced the food-paper, and vice versa. Aside from the odor thus taken into account, the odor of the pigments of the two papers might presumably aid discrimination, hence within the paper in the food-glass, we placed a cylinder or lining of the confusion-paper and likewise within the paper in the confusion-glass was placed a cylinder of the food-paper. Moreover, the various papers were kept in one drawer and thus any odor peculiar to a certain pigment must have been pretty well diffused through them all.

There seems to be an almost irrefutable proof that our precautions were successful in at least a large number of cases. This proof rests in the fact that we obtained many complete confusions and, as already stated, each confusion was controlled by second cat or follower, which made it certain that the confusion was not due to accidental circumstances nor to some individual peculiarity of the first cat.

In working through the series of Bradley colors we came gradually to those among which confusion was difficult (required at least two hundred forty trials to attain twenty-four correct choices in a series of thirty trials). Then we came to colors which were confused with the food-color. We shall call these groups of colors, respectively, areas of "difficult-discrimination" and "confusion-areas," though in some cases these "areas" overlapped, i. e., one color was confused with the stimulus color, the next was discriminated from it with difficulty, the next was confused with it etc.

Tables I, II, and III show typical cases of discrimination, difficult discrimination, and confusion.

TABLE I (A)

Typical Discrimination by Series

Cat 2

Stimulus Color—Yellow

Yellow with	Errors	Correct Choices	No. of trials
B	0	30	30
BT1	0	30	30
BT2	5	25	30
GBS2	0	30	30
GBS1	0	30	30
GB	0	30	30
GBT1	0	30	30
GBT2	11	49	60
BGS2	0	30	30
BGS1	0	30	30

TABLE I (B)

Typical Discrimination by Series with Approach to a Confusion Area

Cat 4*

Stimulus Color—Yellow

Yellow with	Errors	Correct Choices	No. of Trials	F. E.	Verdict
YGS2	0	30	30	10	Discrimination
YGS1	0	30	30	7	"
YG	6	24	30	3	"
YGT1	5	25	30	3	"
YGT2	14	46	60	7	"
GYS2	1	29	30	8–9	"
GYS1	4	26	30	5	"
GY	38	82	120	3	"
GYT1	207	393	600	2	Confusion
GYT2	207	393	600	1–2	"

TABLE II

A Typical Case of "Difficult Discrimination"

Cat No. 3

Stimulus Color, Blue. Confusion Color, VBT1

Series	Errors	Correct Choices	Per Cent of Error
1	11	19	$36\frac{2}{3}$
2	10	20	$33\frac{1}{3}$
3	14	16	$46\frac{2}{3}$
4	14	16	$46\frac{2}{3}$
5	9	21	30
6	15	15	50
7	13	17	$43\frac{1}{3}$
8	8	22	$26\frac{2}{3}$
9	10	20	$33\frac{1}{3}$
10	5	25	$16\frac{2}{3}$
11	11	19	$33\frac{1}{3}$
12	8	22	$26\frac{2}{3}$
13	14	16	$46\frac{2}{3}$
14	9	21	30
15	10	20	$33\frac{1}{3}$
16	12	18	40
17	8	22	$26\frac{2}{3}$
18	5	25	$16\frac{2}{3}$
19	12	18	40
20	5	25	$16\frac{2}{3}$
21	7	23	$23\frac{1}{3}$
22	12	18	40
23	14	16	$46\frac{2}{3}$
24	3	27	10
25	4	26	$13\frac{1}{3}$
26	5	25	$16\frac{2}{3}$
Total........	248	532	Average...... $31\frac{7}{26}\%$
Grand Total.	780		

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*This cat had discriminated the yellow from the Bradley colored papers, beginning with the Red Violets through to the place where this table begins, i.e., he had discriminated it from forty Bradley colors.

TABLE III

A Typical Confusion

Cat No. 4

Stimulus Color, Yellow. Confusion Color, Gray 1-2*

Series	Errors	Correct Choices	Per Cent of Error
1	15	15	50
2	14	16	46⅔
3	18	12	60
4	13	17	43⅓
5	12	18	40
6	19	11	63⅓
7	15	15	50
8	15	15	50
9	14	16	46⅔
10	16	14	53⅓
11	15	15	50
12	17	13	56⅔
13	14	16	46⅔
14	13	17	43⅓
15	15	15	50
16	16	14	53⅓
17	17	13	56⅔
18	13	17	43⅓
19	14	16	46⅔
20	15	15	50
Total...20	300	300	Av. No. Errs..50%
Grand Total.................600			

CONFUSIONS OF COLORS WITH GRAYS

In a recent report on the vision of the rabbit Miss Washburn[8] says, "In order to eliminate the brightness error in experiments on color vision in animals, it is not sufficient to show that the animal tested can distinguish a color from the gray that a color blind human being would see in place of the colors, but the animal must be proved capable of discriminating this color from all grays." For a year before Miss Washburn published her report, we had been searching for a gray which the cats would confuse with orange-yellow, yellow, or blue. In their turns, cats numbered one, two, and three discriminated these colored papers from each of the fifty Hering grays.

Meanwhile some of our experiments had revealed the fact that the cat could not discriminate the yellow from some other colors of the same flicker equivalent. It was consequently suspected that the texture of the Hering papers was furnishing the clue to discrimination. But Hering grays numbered one and two had been discriminated from yellow by one cat only with considerable difficulty and after a long period of training. We then sought to find a gray, of a texture similar to that of the yellow paper and of an intensity near to that of the grays mentioned above. We found a gray, an ordinary writing paper of rather dull finish, which fulfilled these conditions. We shall refer to it as Gray 1-2, since its flicker equivalent was mid-way between grays 1 and 2. This gray was confused with the yellow, as the following tables show.

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*Throughout the paper grays will be referred to as "colors" in this way.

[8] Washburn, M. F. *Psych. Bull.*, vol. 9, 1912, p. 54.

Though very extensive tests were made, we could find no gray paper which was not very promptly discriminated from blue. It occured to us that a very considerable range of gray cambrics is supplied by the dry goods stores. As a last resort we turned to trying these. Most of them were promptly discriminated from the blue, but finally a dark gray shade (F. E. —44-45) was found which neither Cat 3 nor Cat 5 could discriminate from the Bradley blue. Meanwhile, we had found a blue cambric which these cats confused with Bradley blue. For the sake of still further evidence we paired this blue cambric with the gray cambric. The two were, in turn, confused.

Again, in using violet as a stimulus color it was found to be confused only with a gray cambric of F. E.—48.

The following tables record the confusions of yellow, blue, red, green, and violet with grays.

Confusion of Yellow with Gray 1-2

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 2	275	325	600	46.8
Cat 4	300	300	600	50.0

TABLE V (A)

Confusion of Blue Paper with Gray Cambric

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 3	283	317	600	47.1
Cat 5	280	320	600	46.6

TABLE V (B)

Confusion of Blue Paper with Blue Cambric

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 3	287	313	600	47.8
Cat 5	280	320	600	46.6

TABLE V (C)

Confusion of Blue Cambric with Gray Cambric

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 3	285	315	600	47.5
Cat 5	274	326	600	45.6

TABLE VI

Confusion of Bradley Red with Bradley Black

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 8	273	327	600	45.1
Cat 3	271	329	600	45.1

TABLE VII

Confusion of Bradley Green with Dark Cool Gray*

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 4	272	328	600	45.3
Cat 9	301	299	600	50.11

TABLE VIII

Confusion of Bradley Violet and Gray Cambric. F.E.—48

	Errors	Correct Choices	No. of Trials	Per Cent of Error
Cat 8	278	322	600	46.3
Cat 9	292	308	600	48.6

TABLE IX

Summary of Confusions of Colors with Grays

Color	Gray Confused with the Color	F.E. of the Color
Yellow	1– 2	1– 2
Green	9–10	6
Violet	48	16–17
Blue	44–45	21
Red	50	25

The following curves show graphically the flicker-equivalents of the colors and the numbers of the grays with which they were confused. If Ives's results could be generalized to cover our flicker tests, these curves would represent the relative brightnesses of the colors for the cat and for the human eye.

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*This gray was somewhat faded. F.E.—9-10.

Thus far we have shown that, the colors, yellow, green, violet, blue, and red have each been confused with a colorless paper (or cambric) by the cats.

Each confusion was found to be identical for two cats.

In each case inability to discriminate was shown by six hundred trials, during which there was no increase in the proportion of right choices. (This is not shown in the above tables but it is shown clearly in our records by series of trials).

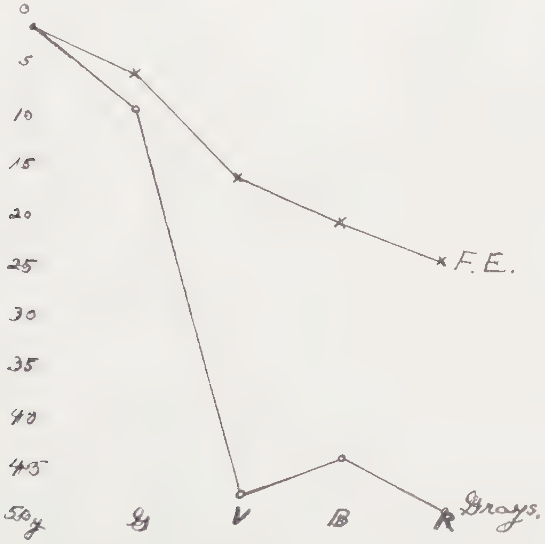

FIGURE 2

The confusions were made in each case after the cat had discriminated its food-color from a number of other grays.

The colors, blue, red, violet, and green were each confused with a gray which is much darker than the gray which represents the brightness value of that color for the human eye.

The yellow was confused with a gray of identical flicker equivalent.

Conclusions: It seems probable that cats cannot distinguish any one color from all the shades of gray, under light adaptation. It is possible that the animal may be totally color-blind by daylight.

Our results in the case of red seem to agree with those of other investigators, namely that red is of low stimulating value for the animals studied. But we may go further and say that blue, and violet are also of low stimulating power for the cat. This suggests the possibility of a much shortened spectrum (i. e. gray band). Yet blue is not confused with black by the cat.

CONFUSION OF COLORS WITH COLORS

Since our animals have confused colored papers with grays, even when the textures were as different as those of paper and cambric, they ought presumably to confuse many pairs of colors, all chosen from the same series of papers. To ascertain whether they would do so we tested each cat by presenting for its choice the stimulus color paired in turn with each of the eighty-nine remaining Bradley colors. In each case the behavior of one cat was confirmed by giving the same tests to a second one. As in the work with grays we began with yellow and blue as stimulus colors.

In the following tables we have included only cases of confusion and of difficult discrimination. The stimulus color was discriminated from all of the Bradley papers not named in the tables, and such discriminations were prompt, with but few exceptions. Examples are given in TABLE I (A). The confusions in each table are recorded in the order of their occurrence in the experiments, but it must be remembered that discriminations, both easy and difficult, intervened between the confusions in many cases.

TABLE X (A)

Confusions Made by Cat 2

Stimulus Color Yellow. F.E.—1-2

Yellow with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
GYT1	374	436	810	2	46.8
GYT2	274	326	600	1–2	45.6
YT1	286	314	600	1–2	47.6
YT2	267	333	600	1–2	44.5
OY	264	336	600	2	44.0
OYT1	276	324	600	2	46.0
OYT2	294	306	600	1–2	49.0
YO	236	364	600	4	39.3
YOT1	266	334	600	2	44.3
YOT2	293	307	600	1–2	46.8
OT1	230	280	510	3	45
OT2	298	182	480	2	60.2

TABLE X (B)

Confusions Made by Cat 4

Stimulus Color, Yellow. F.E.—1-2

Yellow with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
GYT1	207	393	600	2	34.5
GYT2	207	393	600	1–2	34.5
YT1	197	193	390	1–2	50.5
YT2	157	233	390	1–2	40.2
OY	172	128	300	2	54.4
OYT1	193	197	390	2	49.4
OYT2	293	427	720	1–2	40.7
YO	236	364	600	4	39.3
YOT1	266	334	600	2	44.5
YOT2	293	307	600	1–2	48.8
OT1	230	280	510	3	45
OT2	298	182	480	2	62.0

TABLE XI (A)

Confusions Made by Cat 3

Stimulus Color, Blue. F.E.—21

Blue with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
BT1	257	343	600	8	42.8
VB	238	362	600	21	39.6
BV	250	350	600	13	41.6
BVT1	209	391	600	7	34.8

TABLE XI (B)

Confusions Made by Cat 5

Stimulus Color, Blue. F.E.—21

Blue with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
BT1	260	340	600	8	43.3
VB	158	442	600	21	26.3*
BV	232	368	600	13	38.6
BVT1	199	401	600	7	33.1

TABLE XII (A)

Confusions Made by Cat 3

Stimulus Color, Red. F.E.—25

Red with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
RVS2	283	317	600	23	47.1
RVS1	210	390	600	20	35.0
ORS1	260	340	600	30	43.3
RS2	240	360	600	31	40.0
RS1	243	357	600	25–26	40.5
RT1	240	360	600	10	40.0
VRS2	260	340	600	30	43.3
VRS1	230	370	600	23	38.3
VR	257	343	600	17	42.8

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*It will be seen from this record of VB that an animal may make as high as 73% of correct choices and yet fail to discriminate twenty-four times out of thirty trials, and *this failure is confirmed by the record of Cat* 3 *on the same color.*

TABLE XII (B)

Confusions Made by Cat 8

Stimulus Color, Red. F.E.—25

Red with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent or Error
RVS2	277	323	600	23	46.1
RVS1	219	381	600	20	36.5
ORS2	258	342	600	30	43.0
RS2	247	353	600	31	41.1
RS1	240	360	600	25–26	40.0
RT1	238	362	600	10	39.6
VRS2	257	343	600	30	42.8
VRS1	238	362	600	23	39.6
VR	241	359	600	17	40.1

TABLE XIII (A)

Confusions Made by Cat 9

Stimulus Color, Green. F.E.—6

Green with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
YGS1	230	370	600	7	38.3
YG	241	359	600	3	40.1
YGT1	239	361	600	3	39.8
GS2	290	310	600	8	48.3
GS1	257	343	600	7	42.8
GT1	237	363	600	3	39.5
BG	288	312	600	7	44.6
BGT1	240	370	600	3	40.0
GBT1	261	339	600	6	43.5

TABLE XIII (B)

Confusions Made by Cat 4

Stimulus Color, Green. F.E.—6

Green with	Errors	Correct Choices	No. of Trials	F.E.	Per Cent of Error
YGS1	260	340	600	7	43.3
YG	239	361	600	3	39.6
YGT1	257	343	600	3	42.8
GS2	257	343	600	8	42.8
GS1	230	370	600	7	38.3
GT1	240	360	600	3	40.0
BG	238	372	600	7	39.3
BGT1	290	310	600	3	48.3
GBT1	253	347	600	6	42.1

In all, these tables show a total of thirty-four confusions of a color with a color. This strengthens the probability suggested by the experiments with grays, that wave length may not be a stimulus for the cat. It is further strengthened by the close agreement of the flicker values of the yellow, that of the gray confused with that color, and that of the colors confused with yellow. The gray, the yellow itself and five of the colors confused with it have the same flicker-equivalent, namely 1-2, while the six remaining confusion colors differ very slightly from that number. Other groups of colors differ widely from the flicker values of the stimuli, which agrees once more with the experiments with grays.

The relations of the flicker values of the colors to those of the stimulus colors are shown in Figure 3.

The cats have been found to confuse each stimulus color with a certain gray on the one hand, and with a group of colors on the other. If we should assume that in each pair of these papers the cat has merely seen that gray once more, we may assign to the group the brightness value of the gray with which the stimulus color was confused. The gray values are then represented, Figure 4, by the straight horizontal lines, whose positions probably indicate the "brightness equivalents" of the several colors for the cats, in the sense in which Miss Washburn uses that term (pp. 145 and 146).

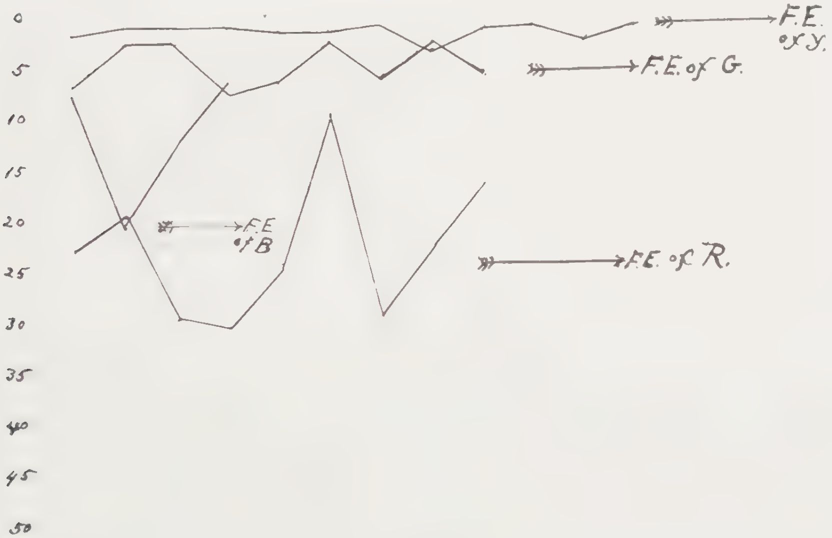

FIGURE 3

The deviations of the curves from the horizontal then indicate how much the several colors may vary, for the human eye, from that gray and yet be indistinguishable from it for the animals. These deviations are large for red and blue, slight for green, and very slight for yellow, as if there the animals approximated the brightness-difference-threshold of the human eye.

The deviations probably show much more the defects of colored papers. They are complex colors and while one factor, say the tint, may presumably brighten the paper, another, say the red or violet, darkens by at least an equal amount. Thus RT1 is very much brighter than R for the human eye, yet the influence of the tint is slight in comparison with the influence of the red for the animal. Hence it is still confused with red. RT2, however, is discriminated from R after many trials, but darkened orange-red is confused. The relations are shown in the following tabulation.

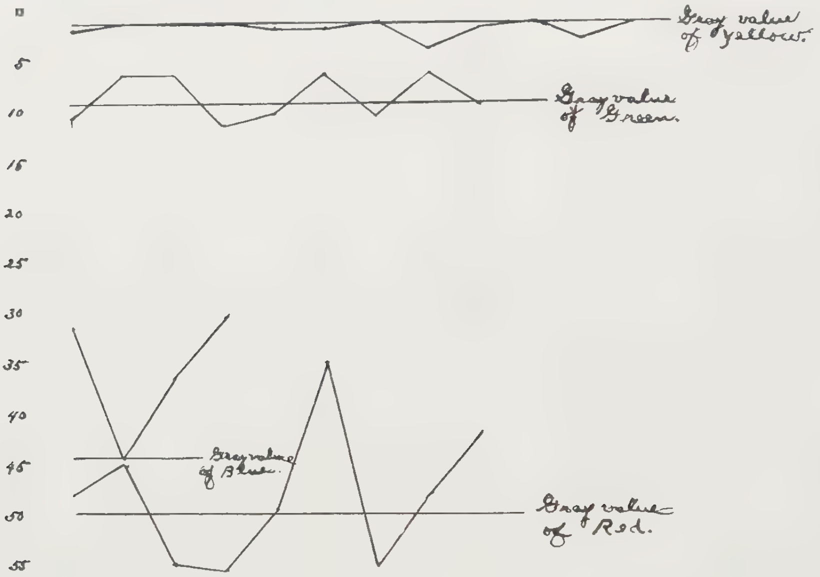

Figure 4

Red with	Errors	Correct Choices	No. of Trials	F.E.	Verdict*
RT1	238	342	600	10	Confusion
RT2	156	444	600	7	Diff. Dis.
ORS2	258	342	600	30	Confusion
ORS1	240	360	600	30	Diff. Dis.
OR	6	24	30	15	Discrimination

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*Based on our standard of twenty-four correct choices out of thirty-trials, as the test for discrimination.

This low stimulating effect of red has been shown for the dancing mouse, the rabbit, and "possibly"[9] for the monkey. Apparently the cat is no exception to the rule. Yet even in the case of red the cat appears to need no such enormous differences in brightness as does the dancing mouse, in order to discriminate promptly. Nevertheless it would be quite unfair to base an opinion of the cat's discriminating ability on his reactions to red.

DISCRIMINATIONS

Our account of the confusions made by the cats is complete. Only matters of minor importance are shown by the discriminations. To our surprise no trace of individual variations appeared in the confusions. This is more apparent because of our determination to employ so many trials that the results would not be vitiated by improvement due to training.[10]

Individual differences did come to light in what we have called difficult discriminations, i. e., those which required at least two hundred forty trials for the animal to learn to discriminate. The greatest difference between any two animals appears in certain colors presented with yellow as the stimulus color. It is indicated by the number of trials required for discrimination by each animal and is shown in the following table.

TABLE XIV

Yellow with	No. of Trials for Cat 2	No. of Trials for Cat 4
G	240	30
GT1	330	30
GT2	360	60
YG	330	30
OYS1	60	330
O	630	630
ROS1	240	240
RO	480	480
VRT2	210	150

There are four prompt discriminations by one cat, only one by the other. The great difficulty of discriminating orange from yellow shown by both animals indicates that it must appear to them very much like the yellow. In this case stopping after five hundred seventy trials would have resulted in a decision that orange is confused with yellow. The per cent of right choices of orange was but 66.2, of VRT2 by Cat 4, only 60.7.

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[9] Watson, J. B. Some experiments bearing on the color vision of monkeys. *Jour. Comp. Neur. and Psych.*, vol. 19, 1909, p. 19.

[10] See Yerkes, The Dancing Mouse, pp. 127 and 128.

There were six cases of difficult discrimination with blue, four with red none with green.

It remains to present the colors which, in experiments with yellow as a stimulus color, were promptly discriminated from it and yet were of nearly the same flicker value as the yellow. It will be interesting to set down in parallel series the colors confused with yellow, those discriminated from it with difficulty, and those easily discriminated from it but of similar flicker value, and underneath each color the number denoting its flicker equivalent.

Stimulus Color, Yellow. F.E.—1-2

1. Confused

GYT1,	GYT2,	YT1,	YT2,	OY,	OYT1,	OYT2,	YO,	YOT1,	YOT2,	OT1,	OT2
2	1–2	1–2	1–2	2	2	1–2	4	2	1–2	3	2

2. Discriminated with Difficulty

G,	GT1,	GT2,	YG,	OYS1,	O,	ROS1,	RO,	VRT2
6	3	1–2	3	5	6	15	9	5

3. Discriminated Easily

RVT2,	VT2,	BT2,	GBT2,	BGT1,	BGT2,	YGT1,	YGT2,	GY,	ROT2,	ORT2,
2	3	3	2	3	2	3	1–2	3	2	3

The first two series of flicker-equivalents show that flicker values were a factor in producing confusions and in causing difficulty of discrimination. In the second series the presence of orange, which we have already seen to be almost as bright as yellow for the cat, is a factor which produces difficulty in four cases. In the case of GT2 the extreme brightness of the tint is overcome by the darkening effect of the green, though they come near balancing each other.

In the third series of flicker values the effect of red, violet, blue, green, and orange in darkening the tints for the cat is very evident. This would indicate that those colors have great darkening effect and that fact confirms to a great degree the brightness position we have given them in Figure 2, which is drawn from the results of grays.

If further evidence of the opposite effects of antagonistic factors is needed, we may take the case of GY. Though the flicker value differs slightly from yellow the presence of green enables discrimination. Add a "tint" and confusion takes place. Brighten the color to the next brighter tint and the result is still confusion. Thus,

Color	F.E.	Result
GY	3	Discrimination
GYT1	2	Confusion
GYT2	1–2	Confusion

Here the effect of the tints overbalances the effect of the green and produces confusion. Elsewhere the darkening effect of the colors is so enormous that the tints and shades have relatively slight effect on the cat. This was to be expected, of course, if the curve derived from experiments with grays is approximately correct. Altogether the results derived from experiments with grays and with colors are surprisingly consistent.

USEFULNESS OF FLICKER EQUIVALENTS

The great drawback of our experiments was the time they consumed in trying so long a list of colors. Could this, by any means have been shortened ? In our work with colors had we tested for yellow only those of its flicker equivalent, we should have discovered five of the twelve confusions. Had we explored one-half of a flicker unit on each side the yellow, we should have discovered five more confusions, and had we explored for two and one-half units on either side, we should have found them all. To have done the same thing with blue we should have had to explore a range of fourteen units, but using only the exact flicker equivalent would have found for us one of the four confusions with blue.

Exploring a range of three flicker units on either side the green would have discovered all the confusions. Using the exact flicker value of the green would have revealed one confusion. Using the *exact* flicker-equivalent of red would not have yielded any one of the confusions, (It is true only of red.) but using one half of a flicker unit on either side of the red would have brought out one confusion. Two flicker units would have shown three of the nine confusions, five units would have shown six, fifteen flicker units would have shown them all.

Even exploring a range of fifteen flicker units on each side of every stimulus color would be a very different affair from pairing each stimulus color with each of the eighty-nine other Bradley colors. Such procedure in the experiments with these cats would probably have reduced the time required from twenty-eight months to eight. An animal which discriminates the stimulus colors from all those of the same and nearly the same flicker values has a very different type of vision from that of the cat.

Though the colors have vastly different brightness values for the cat and for the human eye, the brightness equivalents and the flicker-equivalents have intersected many times. A claim that the use of flicker values would not save time must assume that four broad bands of flicker-equivalents on either side of yellow, blue, red, and green respectively might none of them meet, at any point, the brightness values of the animals. Such an assumption is so improbable that in work with colors the neighborhood of the flicker-values of the stimulus colors should be explored first, provided the animal has already made a number of discriminations to become accustomed to the experiment.

In the experiments with grays, yellow was confused with a gray of the same flicker value. To find the confusion gray for green, we should have had to use a range of four flicker units. With red, blue, and violet flicker values would have been useless for discovering the grays, and "systematic groping" such as we have used in our experiments would be necessary in finding the gray values of those colors for the cats.

GENERAL REMARKS

We frequently tried the cats with two glasses lined with the same colored paper, e. g., two yellows, two blues, etc. They failed, so that the assumption that they distinguished by wrinkles or spots on the paper is gratuitous.

Our records show practically a dead level of uniformity in the responses of each pair of cats. It seems hardly necessary to confirm the work of one cat by giving the same tests to another. It adds a trifle of reliability, but it has added but one new fact to our results, and that of slight importance.

This uniformity of behavior suggests also a dead level of stupidity. A glimmer of intelligence was observed, for, as already stated, one cat gave good evidence of selecting the food-glass by the position of the thumb-button at the rear of the apparatus, and as good evidence of failing to do so when the buttons were concealed by shields.

Our experiments show that the cat has very defective daylight vision as compared with that of human beings. Is it possible that this defective vision accounts for the behavior of Thorndike's cats which clawed at the place where the loop had been when the loop was no longer there ? For such vision as the cat possesses, a mad scramble would conceivably be a much quicker way to lay hold of a loop than an attempt to see it. An accident of similarity of brightness between the loop and the background might render it well nigh invisible to the animal. Is it possible that the poorer the vision an animal possesses the more he becomes dependent on kinaesthetic sensations, which Watson has shown to play a fundamental role in the life of some animals.

Our records show that an animal may make more than fifty per cent of right choices throughout a large number of trials and yet not learn to discriminate between the two objects.

Our experience shows that the possibility of the texture error should be guarded against, as well as the error due to improvement by training. In some cases discrimination occurred only after eight hundred trials.

So many criticisms have been made of the use of colored papers that one advantage in using them, no matter how trifling it be, should be welcome. All the confusions made by these cats can be exhibited to the eye by pasting the papers on gray cardboard. The result of viewing the papers will be a better conception of the nature of the cats' vision than can be got from reading pages of description of their behavior in the experiments.

Finally we asked two persons of dichromatic vision to sort these colored papers as Holmgren worsteds are sorted. Each of the dichromates made five confusions which had been made by the cats. Both of the dichromates and the cats agreed in the matches (confusions) of two pairs of colors, and for each of these pairs the flicker-equivalents were identical.

Our account of our exploratory tests of the cats' vision is finished. We hope that feline vision may now be studied quantitatively, by means of apparatus which permits of accurate measurement of the wave-lengths and intensities of the lights, as they reach the eye of the animal.

THE WHITE RAT AND THE MAZE PROBLEM

II. THE INTRODUCTION OF AN OLFACTORY CONTROL[1]

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What part has olfaction in the life of a rat? The answer to this query would have to be based upon what we know of brain structure and from our casual observation of rat behavior, since, there has been very little direct experimentation published that has as its main concern this form of sensitivity.

The rat has well defined olfactory lobes and tracts. But these parts are relatively smaller than those of some other rodents and decidedly smaller than those of some other mammals. The olfactory paths in the brain of the rat have not had much study and we are thrown back, therefore, upon what we know of the life and habits of the animal for the answer to our question.

It might be thought, from watching the reactions of the rats in the maze, that smell was a very important sense. The frequent sight of a rat lifting itself on its hind feet and sniffing vigorously, the constant use which it makes of its nose on the floor and sides of the maze, would lend credence to such a supposition. Yet it has been shown that anosmic animals are under no serious disadvantage in learning the maze and that much of this sniffing and apparent smelling has an important tactual function. What the world of odor is to a rat we have little power of conceiving but how it affects the behavior we may somewhat discover.

The odors which are vital in the animal world are, presumably, food odors, sex odors and body odors. By the term body odor is meant those olfactory qualities which perhaps are peculiar to individual animals but which certainly characterize the animals of a single cage or group. By differentiation from this familiar odor it serves to mark off a strange animal or give warning of an enemy.

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[1] This work was done in the Psychological Laboratory of the University of Chicago. I am greatly indebted to the department for the opportunity to do it and to professor Carr for suggestive help and criticism of both experimentation and paper.

Rats are omnivorous and hence there can be slight necessity for any fine discrimination in the way of foods. A generalized response to food odor will be all sufficient. I have, indeed, never seen in white rats any clear discrimination of foods which might be said to depend upon smell and have failed to find any mention of such power by others. If food be introduced into a cage unobtrusively, a rat usually stumbles over it before discovering it. It might be supposed that blind and normal rats would show different behavior in food seeking, yet in some preliminary experiments covering several weeks, the food in every instance, by both normal and blind rats, was apparently found accidentally. The animals were very tame and were very hungry. The food used was nuts, cheese and milk soaked bread. The experiments, although significant, were too brief to be conclusive. The instances which Small[2] cites of the reactions of very young animals to different odors may clearly depend upon the chemical sensitivity of the mucus membrane of the nostrils and must be sharply distinguished from olfaction proper. Professor Watson,[3] however, found that blind animals, otherwise normal, were affected by odors to which anosmic animals failed to respond. To repeat, smell is more closely associated with food getting than is any other sense; yet it may be safely assumed, and we should expect to find, that the sense is less refined in animals which do not pick and choose their food than in those which do.

If a rat from another group is introduced into a cage containing other rats they "nose" the whole body of the stranger. The rats do not appear to get the odor across the cage for the excitement and characteristic actions begin only with contact. Rats also respond by different behavior to strange handling. No doubt a large part of the excitement is due to different methods of lifting, etc.; but after the emotional disturbance is allayed the "nosing" of the hand seems to indicate an odor stimulation also. The power to follow a trail is usually supposed to depend upon slight traces of body odor which remain upon the path which an animal has taken. Animals which do not prey upon others for food have little need for tracking. Experimentation has failed to show such ability in these animals.

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[2] Small, W. S. Notes on the psychic development of the young white rat. *Am. Jour. of Psych.*, **11**, 89.

[3] Watson, J. B. Kinaesthetic and organic sensations, etc. *Psych. Rev. Mon. Sup.*, **8**, no. 2, p. 65.

Sex odor calls forth specific behavior. This odor, however, does not seem to carry from cage to cage even though the cages are placed side by side. Efforts to establish the tracking of one sex by the other have been made[4]. Watson said he found no good evidence of tracking but that adult rats showed preferences for entrances that contained the odor of the opposite sex.[5] Small insists that he had no evidence to show that the males followed their own tracks or those of other males or that females followed the tracks of the males.[6] Possibly these attempts have not been made at the right periods; at least the results are inconclusive.

How well rats or other animals can localize odors is still an open experimental field as is also the possibility of olfaction functioning in giving distance values.

The object of this work was to see whether an olfactory control could be introduced into the learning of the maze, and, if it could be, to discover how it would affect the learning process as compared with other forms of control.

The modified Hampton Court maze was used, the same one which served for the experiments with vision.[7] Before beginning the work, the inside of the maze was heavily coated with white enamel paint to cover and to destroy any previous odors, and upon the floor of all of the runways were laid long strips of heavy white paper. The paper was cut 4 in. in width and where the strips overlapped they were fastened with gummed paper. Upon this papered floor was rubbed in, down the center of the runways, a narrow trail of alternating beef extract and cream cheese. It was thought better to use two substances in order to guard against a possible olfactory fatigue. The trail was laid upon paper because of the ease with which such a covering could be removed in varying the experiment and because of a desire to avoid a permanent odor in the maze.

The rats used in this work were young, untrained rats about

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[4] Watson, J. B. Animal Education, p. 51.
Small, W. S. Experimental study of the mental processes of the rat. *Am. Jour. of Psych.*, **12**, 232.

[5] Op. cit., p. 53.

[6] Op. cit., p. 213.

[7] Vincent, S. B. Vision in the maze. *Jour. Animal Behav.*, **5**, 1.

90 days old. They were fed in the maze and handled for a week preceding the beginning of the real work. During the experimentation they ran the maze three times a day under the stimulus of hunger and were amply fed at the conclusion of each day's work. The first experiment was one in which the trail was laid in the true path in the maze and not in the *cul de sacs*.

EXPERIMENT I. Olfactory Trail in True Path

1. Behavior

The behavior in this experiment will be described somewhat in detail since it is significant. There was none of the wild running seen in the usual maze reaction. When put in the box the rats were at once attracted by the odor. Their little noses went down to the trail and they began to follow it immediately. They moved along in a jerky fashion stopping occasionally to smell and to lap the trail with their tongues. This manner of running made their progress an exceedingly slow one. Both the cheese and the beef extract which were used were diluted with water so that there was but a very slight trace of the food on the paper. Still the animals may have obtained some satisfaction in lapping, but such gratification must have been very limited. In general the rats lingered longer over the cheese than over the beef extract trail. The odor was probably stronger. They often hesitated at the places where the trail changed from one substance to another and sometimes struck the "back" or "home trail" here. These returns only now and then resulted in an entrance into a blind alley. They usually ended where the trail changed again. The maze is so constructed that the food box is in the center. When in use, there is always food in this box which the animals are encouraged to smell before the beginning of the experiment and which furnishes their reward when they reach the box at the end of their run. The true path passes directly by the side of this box. (See "Vision in the Maze," Fig. 1.) In the normal maze the early runs are always broken at the food box which the animals have to pass in the center of the maze. The food odor is stronger here and they bite and claw and scratch in a futile endeavor to end the quest at this spot. But notwithstanding the marked early influence of the odor of the food box this behavior, in the normal maze, is very quickly abandoned.

Long before the rats cease to enter the *cul de sacs*, before any of these errors are entirely cut out, the loitering at the food box is no longer to be seen. It only thereafter occurs in exceptional cases where an animal is entirely lost and as a consequence is in a disturbed and emotional condition in which all the old errors reappear. The behavior of the animals following the odor trail, on the contrary, although similar at the food box was more persistent than any "off trail," blind alley error. The odor, it will be remembered, was that of the food with which they were accustomed to be fed. Perhaps the previous stimulation of the olfactory trail had made the animals more susceptible to this influence. But whether, as a result of following a food odor trail, all food odors attracted the attention more, or whether this stronger food odor represented the natural instinctive ending of a food trail and thus called a halt, these are questions for thought. Either or both positions are plausible.

Whatever the cause of this behavior, as a result of it, the speed in all of the early trials was slower than that in the normal maze; but by following the trail the animals were kept in the true path so that the errors were greatly decreased in both the initial and in the succeeding trials.

2. The Tables

Table 1 shows, side by side, the records for the first twenty-five trials in the normal and the olfactory mazes. Figs. 1, 2, and 3, show the curves plotted from these records. These curves are not made like those shown in "Vision in the Maze" because in the olfactory maze the learning period covered less than ten trials and was practically uniform. The units used in plotting were one trial, one minute and one error. Since it was the following of the trail in which we were interested, the error consisted in leaving the track. Returns were not counted and this fact makes these curves comparable with those made for the black-white maze where the returns could not be counted.

The results of this experiment show an increase in accuracy, both initial and total, over the normal maze and an increased final speed. We will consider first the facts which bear out these assertions as to accuracy.

3. Comparative Accuracy

As the table shows, in the first trial, these animals in the olfactory maze averaged only 4.5 errors as compared with 14.7 made in the normal maze. Thus the initial accuracy was three times as great. The final accuracy was greater also. The olfactory maze shows .04 average errors per trial for the last five trials while the normal maze has an average error of .1 per trial for the same five runs. The total number of errors per animal in the olfactory maze is only one-third that of the animals in the normal maze. The error curve, seen in Fig. 1, bears out all of the above statements. Its chief features are the low beginning height, and hence slight fall, and the almost complete low level which it maintains after the twelfth trial. A comparison of the error curve of the normal maze with this will emphasize these facts better than words.

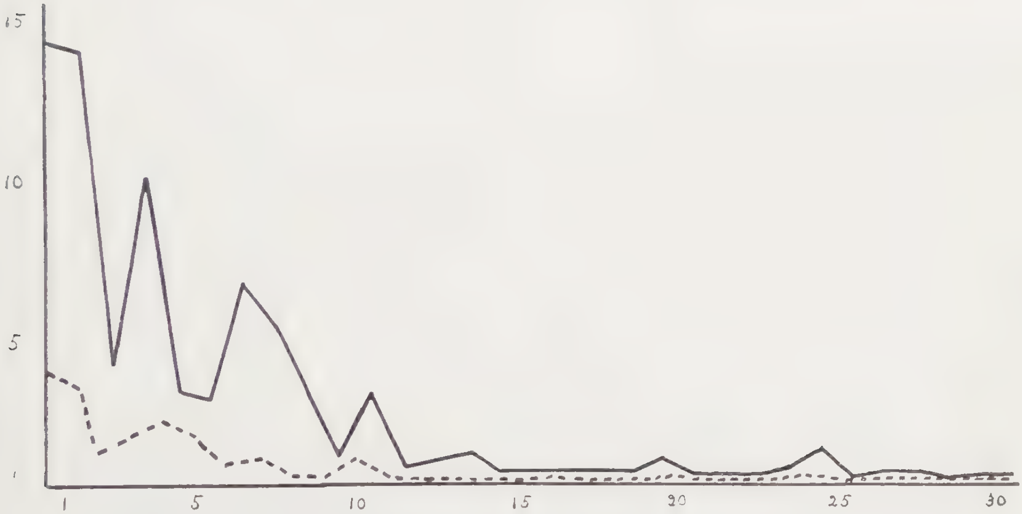

Figure 1. Time and error curves for Experiment I. Olfactory trail in the true path. Full line time, dotted line errors.

4. Speed

The time per run for the early trials was less than in the normal maze as may be seen from the table but this was entirely owing to the fact that there were so few errors. The actual speed was much slower. In the first trial they averaged only 4.5 errors per animal yet the time average is 13.5 minutes. The record for the second trial is practically the same. Almost the same average number of errors, 4.1, was made by the normal animals in the fifth trial in an astonishingly shorter time. For the first trip without error these rats had an average time of 160 sec. The time record for fifteen rats in the normal maze for the first perfect trip is less than one-fifth of this—30 sec. In final speed, however, these animals excel. This maze has an average record of .28 min. for the last five trials as against .31 min. for the normal maze. This is a difference of nearly two seconds—an appreciable difference when one remembers that the maze can be run in ten seconds.

The time·curve (Fig. 1) is very unlike the usual time curve. Compare it with Fig. 3. It is not the beginning height which is remarkable but the persistence with which it maintains this level—the slow rate of elimination of the surplus time. Forty-seven per cent of the surplus time was eliminated in the normal maze in the second trial, in the olfactory maze only 2.5% was eliminated at this time; 80% was eliminated in the first four trials in the normal maze, but it took the rats in the olfactory maze nine trials to reach this point. By the tenth trial the animals in the normal maze had only 2% surplus time left to eliminate, but the rats in the olfactory maze did not fall permanently below this 2% point until the twenty-fifth run.

It must be clearly evident that this olfactory trail was affecting the learning process but before any definite conclusions were drawn it was necessary to put the trail in the *cul de sacs* instead of the true path and to see what would happen then.

EXPERIMENT II. TRAIL IN *CUL DE SACS*

1. Behavior

This experiment was conducted exactly like Experiment 1, with animals of the same age, etc. The only difference was in the trail which was laid from the entrance of each *cul de sac* to its extreme end. There was a noticeable difference in the numerical results as well as in the behavior under these conditions.

The animals in this maze also made fewer errors from the beginning than the animals in the normal maze and the speed was greater also. When put in the maze the rat ran, as in the usual maze, headlong down the runways. Soon he blundered

TABLE I

RECORDS OF THE FIRST 25 TRIALS, TIME AND ERRORS, OF RATS IN NORMAL AND OLFACTORY MAZES

Trial	Average Time in Seconds per Trial			Average Errors per Trial		
	Normal	Olfactory trail in true path	Olfactory trail in errors	Normal	Olfactory trail in true path	Olfactory trail in errors
1	1804	820	991	14.7	4.5	9.6
2	966	800	463	11.9	4.	5.6
3	1043	224	598	10.4	1.1	7.3
4	847	609	331	7.4	1.6	5.3
5	231	175	49	4.1	2.	2.
6	192	165	54	3.5	1.5	1.8
7	64	376	30	1.6	.6	1.1
8	49	295	27	1.4	.8	.5
9	37	178	37	1.5	.3	.5
10	32	52	22	1.1	.3	.1
11	26	155	30	.7	.6	.1
12	25	29	36	.4	.3	.3
13	31	35	32	1.	.1	.1
14	20	52	21	.3	.8	0.
15	32	27	45	.6	.3	0.
16	46	26	83	.7	.1	1.1
17	44	24	97	.5	0.	.3
18	51	25	173	.6	0.	2.1
19	40	23	150	.1	0.	.8
20	32	39	177	.2	.1	1.1
21	31	29	94	.2	.1	.7
22	26	32	262	0.	.3	1.7
23	17	32	117	0.	.3	.8
24	19	37	107	.1	0.	.8
25	22	76	147	0.	0.	.7

TABLE II

TABULATED STATEMENT OF THE RESULTS IN THE THREE MAZES

	Normal Maze	Olfactory trail in true path	Olfactory trail in errors
Average time of learning..	12.1 ±3.6 trials	8.1 ±2.4 trials	7.3 ±3.8 trials
Average time of the first five trials..............	16.3 ±6.7 min.	8.7 ±3.9 min.	8.1 ±5.2 min.
Average speed of the last five trials	.31 ± .05 min.	.28 ± .08 min.	.47 ±.08 min.
Total surplus time........	93.9 min.	64.98 min.	66.45 min.
Average errors first trial...	14.7 ± 7.7	4.5 ± 3	9.6 ± 6.8
Average errors in the last five trials..............	.1 ± .14	.04 ± .04	.44 ± .21
Total average errors per animal.................	66.6 ± 16	20.5 ± 5.6	52.1 ± 12
First run without error....	8.3 ± 3.1	6.3 ± 2.8	7.5 ± 1.8

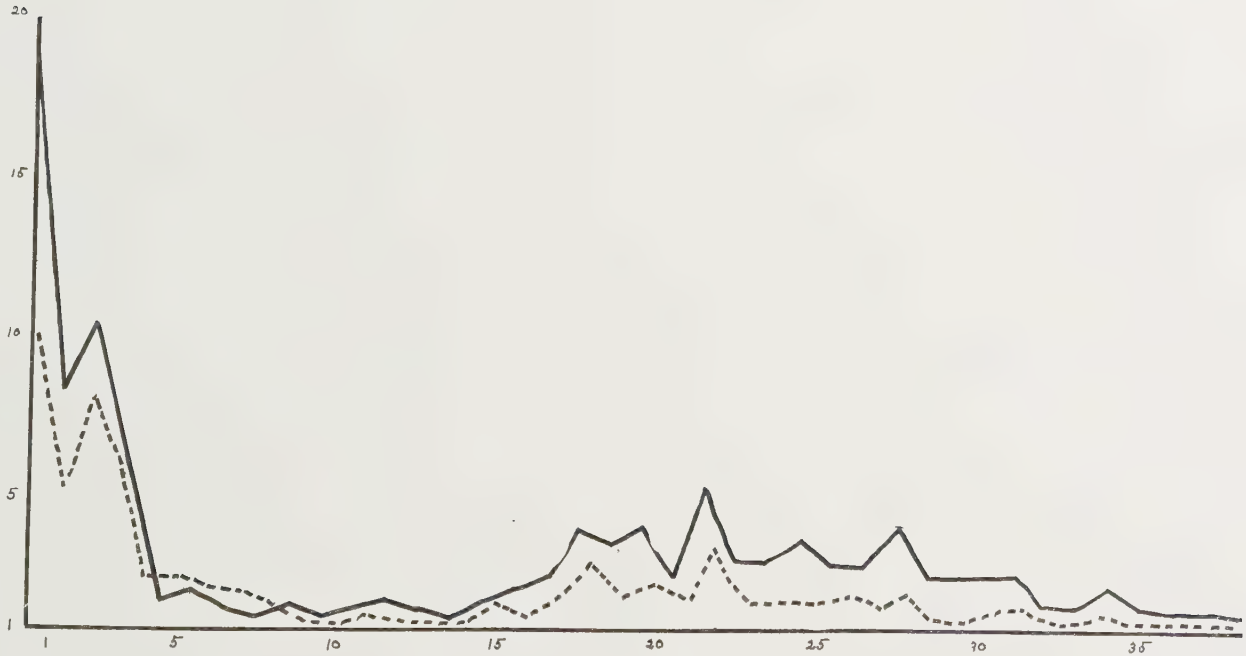

FIGURE 2. Time and error curves for Experiment II. Olfactory trail in the *cul de sacs*. Full line time, dotted line errors

into a *cul de sac* and down went his nose to the trail which he followed for its entire course, to the end of the alley. He moved along by jerks, as described before, and when he reached the end, he turned and in the same irregular, slow, halting way returned to the entrance of the alley. Between the *cul de sacs*, he ran; but when in them, slow movements were the rule. As a result more time was spent in a single *cul de sac* than had been the case in any of the other experiments. Still, from the first, these excursions from the true path were lessened in number as compared with the normal maze. The blind alleys seemed to be marked for the animal in some way. He began to go less and less deeply into them and finally, as he was running more and more confidently in the true path, I have seen him, time and again, actually thrown back on his haunches if chance running flung him into the entrance of a *cul de sac*. Or, he might be running quickly, swerve into an entrance, and there would be seen an instant decisive turning the minute he struck the trail. It looked like a real discrimination. Surprisingly enough, however, after the problem was learned, and the animal was making 90% correct trials, these errors began to reappear and it took almost as long to get rid of them the second time as it did the first. The meaning of this will be discussed later. There were many more returns in this experiment than there were in the one where the trail was laid in the true path—five times as many in the first trial, It was a long time before the rats learned to pass the food-box without lingering. The numerical results for accuracy confirmed the conclusions drawn from the observed behavior.

2. Comparative Accuracy

Under the conditions of this experiment, the accuracy was decidedly greater than in the normal maze in the first fifteen trials. If we now make a comparison with the other olfactory experiment, we find that more errors were made in the first nine trials than were made by the animals which followed the trail in the true path but that the next six trials were more perfect. From the fifteenth trial on, the accuracy was far less than in Experiment 1, or in the normal maze, and it was only toward the end of the experiment, that it again approached their standard. The curve (Fig. 2) shows this variation exactly. The total average number of errors per animal was 20% less than in the normal maze but the animals made two and one-half times as many errors as their brothers in the experiment where the trail was in the true path. The learning time was actually shorter than in Experiment 1. There is so little difference, however, that it may be a matter of chance. The conditions, as a whole, were very favorable for learning as compared with the normal maze and scarcely less so than those in Experiment 1, where the trail was in the true path. It seems fair to conclude, therefore, that these conditions did affect the accuracy, and in general favorably, but that there was a variableness in the final reactions which will require explanation.

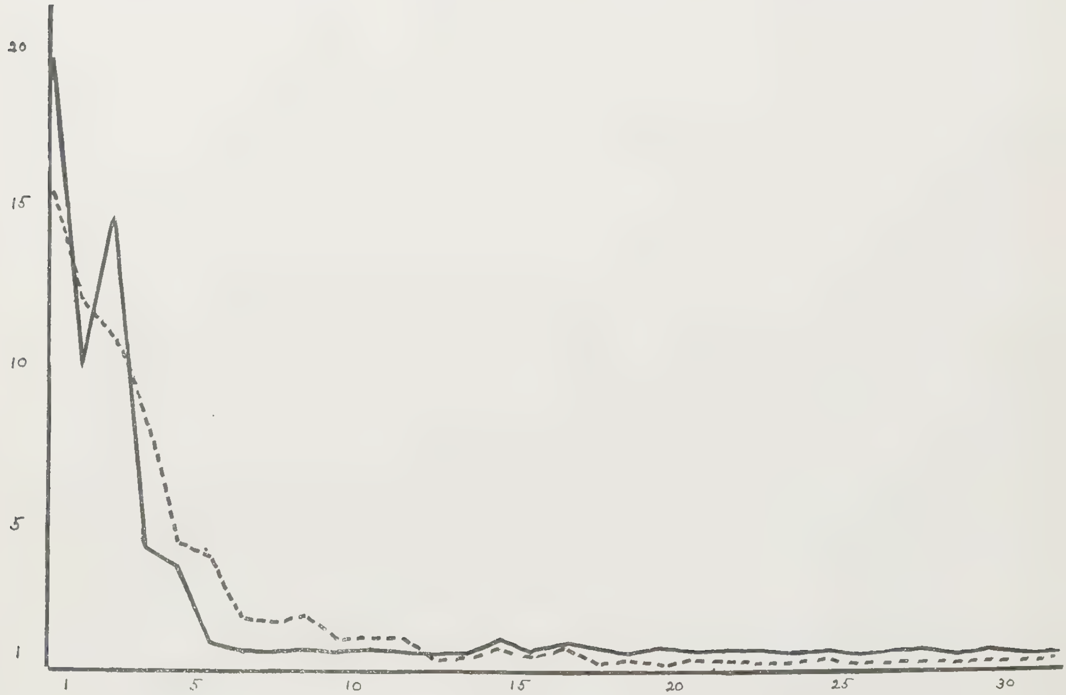

Figure 3. Time and error curves for normal maze. Full line time, dotted line errors.

3. Comparative Speed

The speed in this maze was quite comparable with that in the normal maze except when the rats were in the *cul de sacs* and, because the errors were so few, the slowness in these places placed the animals under only a slight disadvantage. The running was much more rapid than that reported in Experiment 1. The figures in Table 1, giving the time per trial, do not show this since the data for the total distance is lacking. In the first trial in Experiment 1, there was an average of 4.5 errors and 1 return. The animals did not go to the end of each *cul de sac* and the returns were only partial. In this experiment, with trail in *cul de sacs* in the first trial, there was an average of 9.6 errors and 5 returns per animal. The larger proportion of these returns were home returns and the *cul de sacs* were explored to their farthest limits. According to these figures the time should have been three times as long in the latter case had the speed been comparable, instead of which it is practically the same (See Table 1). From this we should conclude that the speed was three times as great in the first trial in Experiment II as it was in the same trial in Experiment 1 where the trail was in the true path. There was a variability of speed in the middle part of the experiment which clearly depends upon the increase in errors. (See the curves Fig. 2). The average speed of the first trial without error may be taken as a point of comparison as we do not possess the figures for the total distance. The normal maze gives us an average of 30 sec. for this trial, the maze with the trail in the true path 160 sec., and this one 28 sec. At this point, then, in this experiment, we have a speed which is quite as fast as that in the normal maze. The final speed, however, within the limits of the experiment, was less—.47 min. (See Table 2). Whether longer experimentation would have developed a speed equal to that in the normal maze, or whether the conditions would always have mediated against it is a question for discussion.

EXPERIMENT III. TRANSFER OF TRAINING

This experiment was the crucial one. The conduct of the rats had been affected by the olfactory trail in the maze, the learning had been aided, but had the animals really gained anything which they could carry over to another problem? Was an olfactory control so well established that it could be utilized in another situation? It was determined to take the animals, at the conclusion of Experiment 1 on the maze, over to a problem box. This box had three runways, leading from a common entrance, and they terminated in a food-box. The paper trail, some of the original paper from the runways, could be laid along these runways, changed in irregular order, and the rats tested here. Before taking them to the box, however, after the conclusion of Experiment 1, the paper was entirely removed from the maze and the rats given one trial each on the maze itself. They made perfect runs showing no hesitation whatever. They did not seem to miss the paper at all and even incorporated in the runs a slight "slowing up," as had always been the case at the places where the trail changed from beef extract to cheese. Evidently the control had become kinaesthetic. The question we had to face now was this: Had the olfactory experience persisted notwithstanding the change of control.

The rats were now taken over to the box. The first trials here gave entirely negative evidence. The olfactory trail might as well have been absent for all the attention which the rats gave to it. The path with the trail was only taken on an average of six times out of twenty trials. The next morning the animals were tried again and then it was seen what they were doing. No matter where the trail was laid, they were always making a straight run to the left and down the runway on the left side. Now this was just their first run in the maze. Clearly kinaesthesis was at the helm and olfaction had retired from the engagement. It was necessary, therefore, to arrange conditions such that the opportunity to make this run to the right or to the left should be done away with—a condition in which *position* so far as possible should be eliminated.

A long rose box, about three feet in length was procured from a florist and in its end were inserted long, heavy, pasteboard mailing tubes. These tubes just filled one end of the box. They were lined with paper taken from the maze and one tube contained paper on which was the trail. In the experiment the tubes were alternated according to an irregular schedule. For the next few days the rats were tried out in this box. When they were put in at the end farthest from the tubes they immediately ran down to these exits. The two openings were side by side, there was no chance to turn, and in fifty trials they made 90% correct choices: i. e., they followed the trail nine-tenths of the time. While sitting in front of the tubes the rats could smell either one indifferently so there was usually a momentary hesitation at the entrances and then a dash into one or the other. Sometimes the head was put in tentatively and then came the sudden run through or the withdrawal. The experiment showed, conclusively, that the olfactory experience had been retained and that it could be utilized again. It also showed that the reaction to the original problem had become a matter of habit and that so strong and powerful was kinaesthesis that the removal of the sensory factors which helped to establish it had no effect upon its control. When later the animals were confronted with a problem where turning to the right or to the left was possible the response was in kinaesthetic, or tactual-motor terms. But when the possibility of runs and turns were cut out the effects of the olfactory learning and experience were asserted in a perfectly effectual way. That this was not due to any attractiveness of the trail in itself is shown by Experiment IV.

EXPERIMENT IV. ANOTHER TRANSFER

This was the discrimination test for Experiment II. The same box and the same method was used as in Experiment III. Under these conditions the animals had to choose the path where there was no trail. They did this just as consistently as the others making just as good a record and confirmed in all points the conclusions drawn from Experiment III. The details are not needed here.

DISCUSSION AND CONCLUSIONS

Whatever may be true of rats in their native environment, we agree with Small,[8] that these animals do not usually follow a path in the maze by means of scent; yet, as these results show, they can do so. The evidence here is also against Professor Watson's statement that. "Olfactory sensations have no rôle in the selection of the proper turns in the maze.[9]" This assertion may be quite true of work on the maze as he used it, but certainly olfaction, in the experiments reported in this paper, helped to cut out the errors. Although we have seen no signs of instinctive tracking, these animals will follow an odor trail on first trial and can learn to follow an olfactory trail or to avoid such a trail. If a maze problem presents such a trail the result is an initial and total accuracy which is greater than normal although the final accuracy, when the trail is in the *cul de sacs*, is less. The learning time is also shortened. We should therefore say that such an olfactory control distinctly favors accuracy.

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[8] Op. cit., p. 232.

[9] Kinaesthetic and organic sensations, etc., p. 91.

How shall we explain this increased accuracy ? Was it a result of real sensory discrimination ? It can be explained, as the results in the black-white maze were explained, as being due to the dominance of some particular stimulus. A path, out upon which an animal first runs in a maze, if not alarming, becomes a familiar place—a home place. There may afterward be other such places in the maze, but this is the first one. He runs out from here, returns, goes a little farther, etc., but always with the possibility of the home return. In Experiment 1, the path was associated with a strong odor trail. Departure from this was to go into the unfamiliar and strange. Thus from the first the animal had more of this stimulus and it became increasingly familiar and increasingly dominant. Dominance, as a term here, may be explained in one way as the power of the familiar. It may have other explanations. Rats are seemingly possessed with an instinctive curiosity or tendency to explore; but fighting against this is an innate tendency to keep in familiar or known situations. The familiar or known situation in Experiment 1, was near the odor trail; in Experiment II, it was away from it. If we accept this view the odor stimulus would be powerful enough to keep an animal in the true path if it arose from this path or to keep it from the blind alleys if it lay there. It would work both ways. It would do so by holding the attention to the true path or by catching the attention and so serving as a warning when the animal strayed from the path. The errors would be lessened in either case.

There were actions, however, which seemed to show that this behavior was more than a mere passive affair. I take it that an instant response to a stimulus, when not instinctive,—a response which can be learned and which can be varied, now positively and now negatively—involves discrimination. There was none of this seen in the black-white maze. There was such behavior here. If this be the case, while the first explanation may be a true and a reasonable one, the increased accuracy here was partly, at least, a result of discriminative ability.

There was an increase of errors in the middle of the learning period in Experiment II, and some slight evidence of the same thing in Experiment I. (See curves Fig. 1 and 2).

The only interpretation 1 can offer is this: It was the result of the changing sensory control. The initial control was dominantly olfactory: but with repeated trials the kinaesthetic experience grew and strengthened and finally began to come into its own. The running became easy and rapid and the accuracy was becoming habitual. Attention, now being released from the control of the movement, was free to be attracted by the olfactory trail in the *cul de sacs* and errors became more frequent. The final elimination may have been, and probably was, a relearning with kinaesthesis more firmly established. But besides accuracy there is also speed to consider.

The conditions of the two experiments give results which differ radically here. As compared with the normal maze, Experiment 1 showed slow initial speed and quick final. Experiment II showed quick initial speed and slow final. Let us first discuss Experiment II.

There is no need to take much time here to discuss the speed in Experiment II. The true path resembled that of the normal maze and the beginning speed was comparable. The slower final speed was a result of the increase of errors. The variable curve seen in Fig. 3 has the same explanation. But let us turn to Experiment 1, where the facts are better seen.

Olfaction has two uses. First it functions as a distance sense. The reaction in this case is always running—toward food, away from danger. The second function is associated with food-taking. Olfaction is so intimately associated with food-taking that, in man, taste and smell are difficult to disassociate. The point which is here to be emphasized is that when the second of these functions is set up in animals in connection with food it inhibits the first. It seems probable that olfaction furnishes animals with a more accurate criterion of distance than it furnishes man and that the nearness of food, with the consequent increased intensity, is the stimulus for the food-taking reaction and the running ceases or slows up. The one response is anticipatory, as Sherrington says,[10] the other consummatory. The one is a somatic reaction, involving the whole body, the other is visceral and confined to certain organs and segments.

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[10] Sherrington, C. S. Integrative action of the nervous system.

If we now attempt to explain the slowness of the reactions of the rats in the maze in Experiment 1, there are several possible interpretations: First, the slowness may be due to the fact that the odor of the food box which served to initiate the reaction is swamped, overpowered, by the nearer, more potent odor of the trail; or, second, that attention is divided between the two and hence we have the characteristic behavior; third, it may be that the pleasurable feeling set up by the odor of the trail is in itself a deterrent and results in loitering; or fourth, it may be that the nearness and strength of this stimulus does initiate the preliminary instinctive food-taking reactions which of themselves end or modify the distance reactions of running.

As one observed the behavior in the initial trial, there did not seem to be any emotional excitement which would suggest the inhibition of running through conflicting motor tendencies and hence the second explanation is discredited. That the trail odor was the predominating one in the first trial seems probable and that it was also pleasurable. The satisfaction of hunger at the end of this trial, however, must, in all succeeding trials, have played a large part and made the original trail a different more intense, more stimulating trail, a somewhat else, *viz.*, a trail which ended with this satisfaction. Yet still there was the loitering and slow movement through all of the early trials which would lead us to think that the fourth supposition may be a reasonable one. Why, then, did this behavior alter in the later trials? Because of the organization of the whole response into an habitual motor series which only required the odor for the initiation and possible reinforcement of the act. The more rapid final speed, which exceeded the normal, may have been caused by the reinforcement of the kinaesthetic control, now established, by the olfaction of the trail.

Miss Richardson says,[11] "Olfaction may accelerate or retard the learning process; accelerate when the odor is a part of the stimulus connected with the problem—otherwise be disadvantageous." It is easy to conceive that it may have the same effect upon the actual rate of running—that it may result here in a genuine acceleration of speed.

While the main purpose of this work was to establish and to study the effects of an olfactory control in the maze, one of the most interesting features of the results was the proof of a transfer of training. So far as the writer knows there has not been shown before in the animal world, at least in such a graphic way, this change of sensory control from one form to another within a single learning process.

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[11] Richardson, F. R. A study of sensory control in the rat. *Psych. Rev. Mon. Sup.*, **12**, no. 1, p. 68.

THE CHICAGO EXPERIMENTS WITH RACCOONS

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At the University of Chicago, three of Professor Carr's graduate students, Dr. W. S. Hunter,[1] and Messrs. F. M. Gregg and C. A. McPheeters[2] have been engaged in repeating experiments similar to mine on raccoons, with results which are most gratifying to me.

Hunter (p. 46 and beyond) found the behavior of the raccoons as different from that of his dogs and rats as I found it different from the behavior of cats. He was compelled, as a result of his experiments, to give up the mere sensori-motor explanation of the behavior of these animals, nor could he attribute it to the association of motor impulses with a whole situation. Motor attitudes could, he thought, account for the behavior of the rats and dogs. It would not serve for an explanation of the reactions of the raccoons. At the close of my experiments, I, too, was compelled to regard those explanations as inadequate. He found that children and raccoons could respond successfully to a stimulus after a much longer delay than could the rats and dogs. He found for the raccoons a maximum delay of twenty-five seconds. The longest delay that I used was at least six seconds, or possibly nine seconds, if we consider only positive reactions of the animals. He compares the behavior of the raccoon favorably with that of a two-and-a-half-year-old child. Moreover, he admits an idea as a "possible cue" used by the raccoons and the children, as against purely motor or sensory cues, used by the other animals tested, though he prefers to attribute the reactions of the raccoons and those of, at least, the youngest child to "imageless thought."

Now that my experiments have been confirmed so fully I must regard them as established. This seems to me to be an item of progress. The psychology of mammals must now cease to be a mere generalization of the psychology of cats. And two of my former students, Professor DeVoss and Miss Rose Ganson, have recently shown what I believe to be an excellent reason why cats may not be expected to behave the same as animals with less defective vision.

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[1] Hunter, Walter. S The delayed reaction in animals and children. *Behavior Monographs*, vol. 1, no. 1, 1913.

[2] Gregg, F. M. and McPheeters, C. A. Behavior of raccoons to a temporal series of stimuli. *Jour. Animal Behavior*, 1913, **3**, 241-259.

We have, then, been driven from the cover of accounting for all mammalian behavior by the sensori-motor hypothesis alone, and psychologists are free at last to try to learn how animals differ in their behavior, instead of denying all differences. This will help enormously, for it may enable us finally to discover a psychology of the higher animals which can explain as well as deny, which can be taken out of the laboratory and yet bear the light of day and the scrutiny of intelligent persons who observe animals. This we have not had. When you meet an observer of horses, who thinks his horse remembers its home, you do not convince him by denying his statements and the evidence he gives, or by calling him "naively anthropomorphic," or by telling him that he did not record the date of the occurrence, or by hurling at him the anathema of "anecdotal psychologist" with opinions "too trivial for serious analysis or notice". A science which can only deny everything and explain nothing is no science and will never receive nor deserve confidence. It certainly was legitimate in 1898 to start by denying the worth of anecdotes of animals for comparative psychology, but only if by denying them we should eventually find a way to explain them, or at least to explain observations of animal behavior which are made almost every day. Experimental animal psychology is now sixteen years old. Consequently it must soon cease to be a generalization of the behavior of cats and take some step which promises eventually to explain animal behavior. Otherwise it must confess bankruptcy and its inferiority to common sense, and remain a sort of science which cannot emerge from the laboratory and which cannot be believed by the psychologist himself the moment he emerges from it.

I am in no hurry for this science to make progress but I should like to see it *take a direction* which promises something. I do not think that a devoted effort to adhere to an objective nomenclature, or to hang the fate of progress on some word, as behavior, or behaviorism, or forever to deny what many observers affirm, is taking a promising direction. It is true that the professors at the University of Pisa saw Galileo drop the weights, and saw them reach the ground at the same moment, and yet refused to believe the evidence of their senses. Animal psychology which merely denies has had an influence in university circles similar to the influence of Aristotle on the professors at Pisa, but it has gained no such influence upon intelligent observers elsewhere. Instead of denying all psychic traits to animals would it not be better to deny our competence to explain more than the merest trifle of animal behavior? I believe that Hunter's confirmation of my results should give a new stimulus to investigators to devise ingenious new experiments suited to find new facts. That avenue seems more hopeful than a denial that there are new facts to be found, and affirming that animal psychology must become a sort of "organic physics."

It is of interest to observe also that while current mammalian psychology cannot come out of the laboratory, common sense observations continually find their way into it. In this paper of Hunter's, for example, one animal is a "stranger" to another and so pays close "attention" to the latter's movements. Preliminary experiments make his animals "acquainted" with the place and apparatus. The raccoons display a directness and "sureness" in their behavior which defies the mathematics of chance. Their "attention" was "distracted" by "yelling at them at the top of my voice" (P. 71). (A procedure likely to make them fierce beyond recall, and which, perhaps, explains the last statement of Dr. Hunter's paper. It is gratifying to learn that this method of distraction was used only infrequently.) Attention and association are everywhere ascribed to the animals and not the association so accurately described by Thorndike, but association pure and undefined. Surely these are greater and more gratuitous assumptions to make than that a horse remembers his stable, even when distant from it, or that a raccoon remembers the box from which it is difficult for him to escape.

I realize that these remarks will expose me to the charge of being as completely deceived as was Herr von Osten, but his is not my position. My view is that "imageless thought," if Hunter's hypothesis is deemed correct, or, at least sporadic, images if my own explanation is accepted as the simpler one, are perhaps so rare in animal experience that the most refined experiments will be required to discover and identify them; experiments beside which Hunter's experiments, and mine, will pale into insignificance, because of their simplicity.

There is still another reason for hoping that the study of animal intelligence may sometime get beyond the stage of dispute and denial. Dispute and denial are poor material to occupy the time of college students. Long ago I had to give up that kind of teaching ,and occupy myself with the more solid information which we possess, of animal sense organs, because dialectic should be taught in philosophy and not in science. Note the extensive work of Kafka[3], the first volume of which has just appeared.

Dr. Hunter's agreement with me does not end with the facts noted above. He is almost persuaded to credit my experiments in putting the animals through the act to be learned, because he has observed the same sort of behavior in rats. At least he admits my apparent credibility relative to my four raccoons. It would be unfair to him, however, not to state his qualifications. On page fourteen he says: "Now with reference to that type of experiment in which the problem learned is that of working latches rather than climbing into boxes, I believe the data presented by Cole are conclusive, *as far as the facts are concerned. Some* raccoons *at least appear* to learn by being "put through." Whether *all* raccoons would do so is, *of course*, *quite* another matter." (Italics mine.)

The reader may reply, "You can surely get but cold comfort from this admission." It gives you the merest semblance or 'appearance' of credibility with regard to your report on your four animals alone. I at least have not charged you with having invented your records." True enough, but the admission means that Hunter's rats, if they have not made a breach, have at least made a weak place in the blank wall of opposition and denial. The latter is definitely given up. How wonderful is the rat at undermining!

The cold comfort comes from the facts that those experiments of mine have not been repeated at Chicago University. I fear, because their raccoons would not permit it (Hunter p. 86). Now should the Chicago laboratory secure a toothless raccoon what may not become of the credibility, temporarily and with qualifications, accorded me? But what disposition, pray, will then be made of the behavior of Hunter's rats? With many misgivings, therefore, I await the report of experiments which may even now be in progress.

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[3] Kafka, Gustav. Einführung in die Tierpsychologie. Leipzig, 1914.

In instinctive behavior the Chicago raccoons confirmed my observations rather than those of Davis. Yet I am sure Davis's report is correct, despite the authorities quoted against him, for I saw occasional cases of what he observed regularly. We must not be too cocksure in these matters. Remember that Audubon never saw his pet raccoon wash its food in the water beside it (Davis p. 451).[4] Yet that behavior gives to the raccoon both the name "lotor," and the name "Waschbär".[5]

Interpretations: The reader who is familiar with Dr. Hunter's thesis will recognize the agreements I have mentioned between the behavior of my raccoons and those of the Chicago laboratory. Our interpretations of this behavior are entirely different, of course, except that we were both forced to give up the sensori-motor explanation. Forced from that position, I thought the animal might have memory, or at least a few memory images carried in visual terms, hence a visual image. I still believe that this is the simplest, or as some prefer to say, the most "parsimonious hypothesis." Hunter prefers the assumption of "imageless thought" or "sensory thought" to account for the raccoons behavior, and for that of at least the youngest child. This "imageless thought" must be, at least partly, *visual*, for he says (p. 74), "In the present case there seems to be no room for doubt that the object reacted to was the light." The reader must remember that this light had been turned off for twenty-five seconds before the animal was permitted to react to it, in the maximal delays with raccoons, hence the "representative functions," next mentioned. For he continues thus: "Now if a representative function were involved in the behavior of the reagents, *as seems to have been the case with the raccoons and children*, it must in part at least, have been *representative of the lighted box*, because all else—including the three possibilities of movement—was constant from trial to trial, whereas a selective response must needs have an alternating cue". (Italics mine).

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[4] Davis, H. B. The raccoon: a study in animal intelligence. *Amer. Jour. of Psychology*, 1907, **18**,

[5] I have to thank Mrs. R. M. Yerkes for calling my attention to this splendidly appropriate German name for the raccoon, and its superiority to the American name, whose source is not certain.

I know of no way in which light can stimulate these animals except *visually*. And when the animals were permitted to react, it was by means of a function representative (at least partly) of the lighted box. One would think that the simplest escape from this dilemma would be by means of a visual image. But no, it is visual in source or cause, yet imageless in content. We have often been led to believe that sensation gives a rather fundamental content to thought. Perhaps we may now teach that Helen Keller, for example, has both the *content* of visual experience as well as a knowledge of its relations. Loeb[6] has recently given evidence to show that a retinal image produces a brain image, which corresponds with the former point for point. "Diese Tatsachen enthalten aber, wie mir scheint, auch den Nachweiss, dass in Gehirn ein Bild der gesehenen Gegenstaende entsehen muss" (p. 1016). By Hunter's hypothesis all of this image forming apparatus is rather useless, for no mental image arises in the raccoon, nor perhaps in the youngest child, under the conditions of the experiment.

Doubtless it will occur to the reader of Hunter's paper that this explanation of the raccoons' behavior, by means of imageless thought, was in no way suggested to him by his experiments and seems to be a rather foreign addition to his thesis, forced upon him by the milieu or suggested by current discussions of the topic in human psychology. In order to use the concept to account for the results of his experiments he must make the claim (p. 77) that imageless thought is genetically prior to thoughts with images, and he must dismiss the opposite teaching as having "no factual basis" but seeming to be "the result of prejudice or of temperamental leaning." Then the point of origin of imageless thought is placed "at least as low as the raccoon" (p. 77). All this seems a trifle complex to me but the actual advance made is, now that the old explanation has been given up, that the reader may choose what hypothesis he will under the law of parsimony.

Doubtless psychologists will be more interested in Hunter's immediate explanation than in his final one, which I have already outlined. When the conditions of his experiment demanded that the animals go to an electric bulb, whose light had been extinguished some seconds before, in order to execute a successful reaction, the rats and the dogs oriented toward the light, either with the whole body, or at least faced in its direction. They kept this orientation during the period of delay in so many of their correct responses that this "motor attitude" evidently served to bridge the time gap between the disappearance of the light and the release of the animal. Consequently their "motor-attitude" accounts for the success of the rats and dogs. The raccoons and the children did not even face the light in so great a proportion of their successful responses that the "motor-attitudes" hypothesis breaks down completely, as an explanation of their behavior.

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[6] Loeb, J. Die Bedeutung der Anpassung der Fische an den Untergrund fuer die Auffassung des Mechanismus des Sehens. *Zeit. f. Physiol.*, 1911, **25**, 1015-1017.

As a result of this outcome of the experiments, Hunter (p. 80) decides that, "Some intra-organic (non-orientation) factor not visible to the experimenter must be assumed in order to explain a significant number of the correct reactions of the raccoons and all of the successful reactions of the children. These cues fulfilled an *ideational* function." (Italics mine.) And again (p. 72), "As we have indicated, such a mechanism would apply only to the non-orientation cues used by the raccoons and children. The type of function here involved is *ideational* in character. By applying the term "ideas" to these cues, I mean that they are similar to the memory idea of human experience so far as *function* and *mechanism* are concerned. They are the residual effects of sensory stimuli which are retained and which may be subsequently reexcited. The revival, moreover, is selective and adaptive to the solution of a definite problem, and when aroused, they function successfully as a necessary substitute for a definite component of the objective stimulus aspect of the problem." He has already said that the effective component of the stimulus was the *light*. Unless he denies, then, a visual content to this "factor," it is a visual, imageless thought. But since he does deny it a representative content, though it has a representative function, he terms it "sensory thought," though the stimulus has been absent twenty-five seconds in the longest delays of the raccoons. This "sensory thought" then becomes the imageless thought of current discussion, by the genetic reversal of current opinion on that subject that I have mentioned above.

It is interesting to observe how very "similar to the memory idea" is this "intra-organic factor." It is a residual effect of a sensory stimulus. It may be retained and revived, is selective, etc. Elsewhere (p. 69), he describes this factor as "Some *unknown* intra-organic cue non-observable by the experimenter. Our data prove conclusively that some such cue was utilized by the raccoons and the children, ...the nature of such a factor must necessarily be defined at present in *negative* terms." When this statement was written it evidently had not occurred to Hunter to place this negative thing in the positive category of imageless thought. His experiments were completely described before reaching this point. Hence, it seems to me that imageless thought was an afterthought, as an explanation.

On the second page of the paper we find this significant statement. "In the interpretative discussion at the close of the present monograph, we shall be confronted with the possibility that images or ideas *may* have guided the reactions of the subjects. In discussion, we shall assume that there is no necessity that psychology postulate such a representative factor save where successful reactions occur in the *absence* of the stimulus (object) or movement represented." So images *may have been present*. Yet throughout his references to my paper Hunter complains that I did not reach a *proof* of the presence of images. When his experiments were completed, he seems to be in much the same position. Just how an experimenter can give *proof* that animals remember or think, even in imageless thought, I am quite unable to guess. I thought that my animals gave *evidence* of possessing visual memory. Hunter's experiments strengthen this opinion of mine very much.

Like Brehm and all subsequent observers of the raccoon, Hunter has noted the fly-catching activities of this animal. He consequently accords to the "Waschbar" the possession of acute vision. In this he agrees with my report.[7]

Errors: On page eighteen, in re-describing some of my experiments, Hunter says, "a block with a *steeple* was placed in a hole," etc. With absolute confidence I must assure the psychological public that I used no "steeple" in my apparatus.

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[7] Cole, Lawrence W. Observations of the senses and instincts of the raccoon. *Jour. of Animal Behavior*, 1912, **2**, 302.

I have, very rarely, *heard* the word "steeple" used for "staple," but never before have I *seen* it so used in a scientific monograph.

Again, I am regarded as having been "misleading" (p. 86) in my statement that "the year-old raccoons apparently are not quite full grown," for Dr. Hornaday and Mr. DeVry say "that raccoons reach maturity at three years of age." But do Dr. Hornaday and Mr. DeVry mean, therefore, that the raccoon accomplishes but one third of his growth each year, as Hunter seems to interpret them? I cannot believe it. I kept my animals three years and I wish now to re-affirm the statement above. They grew but little after the first year. Work and confinement may have stunted them, though they were fed each day to satiety. In parks I have now seen many raccoons of about the same size which mine attained. They had been in confinement for a long time so they must have been full grown. I have also seen a number of much larger specimens.

Criticisms of my Work: The introduction to Dr. Hunter's thesis takes the form of a fearful arraignment of both my experiments and my arguments. To use his own phrase, most of the latter "can be dismissed summarily" (p. 16). They are in turn dismissed summarily in favor of the sensori-motor explanation, so his theory of raccoon behavior at the beginning of his paper differs entirely from that at its close. I suppose that I ought to make some reply to these criticisms, but I shall be as brief as possible and at that I shall select only the most important ones. It seems better to omit any answer at all to such remarks as, "To some it may seem too *trivial* either for serious analysis or notice" (p. 10), a criticism which I seem to share with Lloyd Morgan and others, save that I have *persisted* in their trivialities.

Criticism 1. "Hence assuming the facts that Thorndike and Cole assume to be unquestionable, it need only follow that the raccoon exhibits more complex sensori-motor behavior than the dog and the cat, and not that it shows a new type of behavior, i. e., a type of behavior involving the functional presence of a representative factor." (P. 15.)

Reply. Yet he later found just such a factor functionally present in raccoons.

Criticism 2. "To argue that this means *image* of apple is certainly naive at least. Could the raccoon not sense the apple when his nose was within a foot of it?" (P. 18.)

Reply. Hardly probable, since the floor between him and the piece was carefully rubbed with another piece of the same apple, and his forepaws were still moist with the pieces of apple he had already eaten. But note Hunter's argument (p. 27) that smell was eliminated in his experiments because the rat was given only a bite, "so almost no food fell on the floor." Food was used with the raccoons in the same way.

Criticism 3. Varying means to the same end. My data under this head are just as inconclusive as that presented above. (P. 17.)

Reply. Curious then that Professor James thought this "the mark and criterion of the presence of mentality in a phenomenon." "We all use this test," says James, "to discriminate between an intelligent and a mechanical performance."

Criticism 4 "The criticisms on Cole's entire work reduce to these: (1) The facts are either inconclusive or irrelevant. And (2) there is no evidence of adequate controls." (P. 20.)

Reply (a) Why then are the same facts, namely, responses to an absent stimulus, so satisfyingly conclusive of imageless thought ? This recalls the remark of Hodgson, "What you know least about, assert to be the explanation of everything else." (I quote from memory.) (b) "No controls." This is the repeated cry in these papers. It seems probable from the statements of the papers that their authors did not read my account and that they misunderstood the few pages they did read. I shall show this in detail in showing that Gregg and McPheeters (and their experiment was planned by Hunter) have entirely misunderstood what I did.

In concluding the discussion of Hunter's report alone the points of similarity between his experiments and mine may be enumerated. He extinguished lamps which were used as stimuli, while I put a series of objects in view of the animal, then out of view again, and he must discriminate, under these conditions, between absent stimuli. Hunter secured delay by caging the animal, while I secured it by not feeding the animal until every member of the series had been put in view and (except the last member) out of view again. Sometimes six objects were used by me (i. e., each of three cards was shown twice). Hunter found it inapplicable to use the third light in many cases.

The second paper, that of Gregg and McPheeters, had no other object than "to demonstrate the inadequacy of Cole's experiment." (P. 258.)

They reconstructed my "card-displayer," except that the levers were not screened from the view or touch of the animal and a system of strings and pulleys was added which the animal could also see.

Then they gave the two raccoons *two days training on the levers alone without any cards attached to them.* (P. 245.) One of the two animals, Jack, failed utterly to discriminate. "Further training *might* have developed discriminative reactions in his case but time did not permit a continuance of the tests." (P. 246.) Jill discriminated between the two series on some basis, but Jill also "soon acquired the habit of standing close to the levers and touching her nose to them as they appeared." (P. 247.) Here, the reader will doubtless say that *all* analogy with my experiment ends. I should agree to this so far as the method and apparatus are concerned but it seems easier to change those than to change the nature of the raccoon, for there is a startling agreement between the behavior of their one successful animal and my four.

Let us find this agreement. In the training series Gregg and McPheeters kept two constant factors. (1) A "normal" order of lever positions used, according to their respective distances from the animal. (2) They always presented the levers in series of three. Jill reacted to the order of lever positions chiefly, perhaps (p. 249), but she responded partly to the threes, for they say (p. 252), "Positive reactions of food getting may be stimulated successfully by any of the following groups, 1-2-3, 1-3-3, 1-2-2, 2-2-2, or 1-1-1. Likewise, inhibition, or negative responses may be stimulated by either group 3-3-3, or 2-2-2. The nature of the stimulus is relative to the character of the group with which it is *alternated.*" One cannot help asking, why continue to alternate by threes only, unless they meant to teach the animal to respond to alternate threes? Why not alternate by sixes as I did? This was one of my "controls," which they have overlooked.

Jill reacted to the two constant factors. In my experiments only the color (and brightness) of the cards was kept constant. My four animals responded to that. In both the Chicago experiments and my own the raccoons responded *to the constant factor*. What more could they do, pray? This seems to me to be an excellent example of the method of agreement.

But this was the behavior of Jill, the single raccoon which succeeded in discriminating in their experiments. One would suppose that no very weighty conclusions would be drawn from the behavior of the animal which failed. But he is said to have responded to the *sounds* of the levers. Their "usual sound." (P. 248.) (Why not make the levers noiseless ?). This animal then responded to sound, perhaps partly to lever order and, I have no doubt, to any other element of the situation which was left constant, and which also enabled him to get food. "He seemed to watch the peep hole, although possibly he was merely listening for some sound upon which to base his reactions" (P. 246), so they set a metronome going to drown the noises made by movements of the experimenter!

I confess I can see nothing in these experiments except a rather determined effort to divert the animal's attention from the cards and to get him to respond to the levers. The following items seem to show this:

1. Two days preliminary training on levers alone.
2. The board screen was reduced to "about five inches" in width (p. 244), thus showing apparently two thirds of the length of the levers, if Figures 2 and 4 (pp. 243 and 247) correctly represent the apparatus.
3. Putting the cards above the raccoon's line of vision, if Figure 4 is correct.
4. Converting my visual experiment into a tactual one by letting the raccoon touch the levers.
5. Adding the cue of noises in operating the levers, as well as noises due to the experimenter's movements.
6. Each of the three strings attached to the levers (Figure 4, p. 247) must have changed from slack to taut *before* the lever appeared, thus further directing the animal's attention to the levers' positions. I am unable to find "controls" against the animals having reacted to the strings.
7. Feeding the raccoon for having reacted to the levers.
8. The colored cards were much smaller than mine.
9. Finally only one of their animals succeeded in discriminating as compared with four of mine.

"The essentials of Cole's apparatus and method were duplicated in our experiment." (P. 244.) Truly, with all these carefully arranged differences, I am quite unable to find that the "essentials" of the experiment were even *similar* to mine, but the reader may judge for himself. Any one who is familiar with my paper will remember that only a small part of the upper portion of the lever projected above the screen board. To be specific, my notes of Dec. 6, 1905, state that the lever "when upright" extended "one inch above the upper edge of the front piece."

"*Controls*" By the charge that I did not employ "adequate controls" is meant chiefly that I did not guard against discrimination by position of cards and levers nor against discrimination by cues given by the experimenter. Let me call attention to two items which my critics have overlooked relative to the first precautions, and quote from notes of the experiments. On page 228, I say, "During one test red would be on the forward lever, one inch in front of the other, during the next test on the rear lever. The animal could not, therefore, react to the position of the cards." I did not re-state this precaution in the portion of my account on which the Chicago laboratory based its experiments, but one presumes that a critic reads completely the paper he criticises. To show that this precaution was kept up during the three-color work I will quote my "daily plan" for one animal for three consecutive days.

"April 23. Jack. Same as preceding. Blue middle, orange back, white front."

"April 24. Jack. Three colors. Orange front, white middle, blue back."

"Jack. April 25. Three colors. Blue front, orange middle, white back."

It is evident that each card occupied every possible position in each three consecutive tests, and that no card occupied the same position for any two tests. Does this look as if I took no precautions against the animals reacting to the positions of the cards? I find no such precautions as this, to leave only the colors constant, in the work of Gregg and McPheeters, so it seems that the animal was fed for depending on another cue.

But the uninformed reader may ask, "But what of *lever* position?" At the beginning of each days work the levers were "strung" on their supporting pivot in any order in which they were picked up. We did not remove them from the room in which the raccoons were kept and we generally found them scattered about the room. The levers were all alike so far as we could detect, until having split one, we replaced it with one having a "new" appearance. This should have brought a new type of result if the animals were responding to the appearance of the levers.

It is true that this change of card position is mentioned briefly at the outset of the experiments instead of within the portion on which the Chicago experiments were based. But was no further precaution taken which was described on the final pages of report ? On page 259, Table 11, I record that for two hundred trials the threes were "shown twice." Since this has been unnoticed or misunderstood let me explain it. It means that I would show red, red, red, red, red, red, and the animal must stay down through it all. Then came white, orange, red, and only then would the animal climb up on the high step to be fed. Thus nine movements were made, and *all the levers were used.* Then followed white, orange, red, and the animal reacted positively and was fed. Thus he could hardly have been responding to alternate threes, or to lever position. Note also that there was an abrupt transition from showing the cards by threes to showing them by sixes. Yet the animal gradually learned to discriminate in this complicated experiment in which all factors were different, except the colors of the absent cards. I describe this showing the cards by sixes at the bottom of page 258, and refer to it as "while you raise three or even *six* colors, again on page 261." Perhaps this detailed account of the precautions taken to guard against discrimination by threes, and against discrimination by position will serve to convince the reader that the experiments were not so careless or hasty as my critics have supposed.

But it is further assumed that I mixed the experiments in which the experimenter manipulated the levers with those in which the animal was permitted to claw at them. The two types of experiment were separated by months. My paper states (p. 233) that no tendency to claw at the levers appeared for *six weeks* of the first type of experiment. After it did appear, clawing at the levers was not permitted until we had learned what we could by the experimenter's operating them. Nor did the raccoons attempt to claw at the levers, if the experimenter manipulated them rapidly. In fact we developed the habit by moving the levers slowly. This confusion on the part of Hunter, Gregg and McPheeters appears to be due to my giving a logical, instead of a chronological account of my experiments.

The behavior of my raccoons was not, therefore due to touch. Consequently Hunter's experiments with lights is more similar to mine than that of Gregg and McPheeters, whose "card-displayer" had some points in common with mine.

As to cues from the experimenter, I always extended my hand as if to feed the animal, at the negative as well as at the positive series. My notes contain many instances, at first, of this response to the hand. These were of course counted against the animal, and finally he ceased to be influenced by the movement. Different experimenters operated the levers and, in one case, it was found that the animals were responding to unconscious movements of the operator. This is mentioned in describing the vision of the raccoon.[8] This experience shows that if the mere presence of the experimenter, or his breathing, had been the cue to which the animals were responding the raccoons would have made far better records than they did, and the work of months would have been reduced to days. I should still prefer to have the experimenter present rather than to use the system of strings, which caused the noise, the peep hole, the opening for food, and to permit the noise of the experimenter's movements, which had to be overcome by a metronome, all of which were used by my critics.

Conclusions: It is noticeable that, so far as Gregg and McPheeters draw a conclusion, they ascribe the raccoon's behavior to "motor attitudes," "sensory attitudes" and, if images were present in our animal, they must have been kinaesthetic, i.e., imaginal attitudes." (P. 258.) Thus they give the explanation of the raccoon's behavior which Hunter found was entirely inadequate to account for it, but which, he believes, does account for the behavior of the dogs and rats. Perhaps, at the time their experiments were made, Hunter's results were still incomplete and it was assumed that the raccoon's behavior would be found in nowise different from that of the dogs and rats. At any rate, we now have three different hypotheses to account for the behavior of raccoons.

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[8] Cole, Lawrence W. Observations of the senses and instincts of the raccoon. *Jour. of Animal Behavior*, 1912, **2**, 302.

1. Attitudes, motor, sensory or imaginal. Gregg and McPheeters.
2. Not attitudes, but imageless thought. Hunter.
3. Visual memory, at least sporadic. Cole.

Truly, "Homines perfacile credunt id quod volunt."